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ABSTRACT

In response to an earlier symposium report, this journal presents six position papers dealing with the question, "What criteria should be used to judge the admissibility of evidence to support theoretical propositions regarding communication research?" The analyses and arguments include "The Research Process" by Wayne Brockriede, "The Reflexivity of Communication Research" by Leonard C. Hawes, "In Praise of Muddleheaded Anecdotalism" by Herbert W. Simons, "Evidence in Communication: We are Such Stuff" by Robert L. Scott, "The Rhetoric of Science and the Science of Rhetoric" by Herbert W. Simons, and "On Keeping the Faith in Matters Scientific" by Gerald R. Miller and Charles R. Berger. (MAI)

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Thematic Issue--Symposium: What Criteria Should be Used to Judge the Admissibility of Evidence to Support Theoretical Propositions Regarding Communication Research?

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Walter R. Fisher
Editor

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Editor's Note

The hope I expressed in introducing the special symposium—Winter 1977 *WJSC*—has been realized. The papers of Gary Cronkhite, Jo Liska, B. Aubrey Fisher, Thomas M. Scheidel, Jesse G. Delia, and Lawrence Grossberg have generated considerable dialogue. Perhaps B. Aubrey Fisher was correct when he observed that the symposium "will undoubtedly solve no issues; nor will it be long remembered," but, at least for the moment, there are those who are aroused and concerned by the matters it explored.

I am pleased to present here further responses to the question: "What criteria should be used to judge the admissibility of evidence to support theoretical propositions regarding communication research?" The responses are various; indeed, in some of the papers, it is difficult to discern clear-cut answers to this question. But that is well and good. The analyses and arguments of Wayne Brockriede, Leonard C. Hawes, Gerald R. Miller and Charles R. Berger, Robert L. Scott, and Herbert W. Simons (who submitted two excellent essays) not only add to the substance of last year's exchange, but also they advance the discussion to new perspectives, new problems, and new topics to be investigated. I am grateful to the contributors for their interest, concern, and willingness to participate in this project. I am sure that their observations will excite more dialogue about significant matters, and this was the idea from the beginning.

WALTER R. FISHER

The Research Process

WAYNE BROCKRIEDE*

I SHALL PURSUE two objectives in intertwining ways in this essay. My intentions are to make some critical comments on the papers included in a symposium published recently in the *Western Journal of Speech Communication*[1] and to make statements of my own about issues addressed by the participants. I shall argue that important to the study of human communication are implications of these ideas: research is a process; philosophical assumptions undergird one's participation in the process; and interpretive philosophical stances are useful perspectives.

Before commenting on these matters, I would like to call attention to a problem confounding all of the essays. No author ever clearly differentiated between philosophical assumptions and a theoretical orientation. Thomas M. Scheidel defined theory as "a conceptual system that serves to explain some phenomenon" (23); he did not discuss philosophy. Jo Liska and Gary Cronkhite distinguished between explanatory and epistemological "theories"; presumably they meant the former to refer to *theory*, the latter to *philosophy*. Their references to "epistemological theory" at times implied a theory one can validate or defend (64) and at times a philosophy one merely assumes (57). B. Aubrey Fisher identified four epistemological perspectives but called them a "theory" (45-46). I read Jesse G. Delia and Lawrence Grossberg as using "philosophical anthropology" and "theoretic orientation" interchangeably (3). I do not regard it a quibble to ask writers to clarify what they mean when they refer to a set of theoretical constructs to explain a dimension of their discipline and when they refer to their fundamental assumptions about the nature of knowledge, reality, and values.

I

That communication is a process is widely accepted, but one sometimes forgets that studying communication is itself a process: it is an activity, a verb. Some persons construe research as an objective and dispassionate search for nonproblematic nouns, for knowledge. This interpretation calls to mind Francis Bacon's metaphor of chasing deed in an enclosed park and

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[1] A symposium, "What Criteria Should Be Used to Judge the Admissibility of Evidance to Support Theoretical Propositions Regarding Communication Research," eds., Gary Cronkhite and Jo Liska, *Western Journal of Speech Communication*, 41 (1977), 3-65. Parenthetical numbers in the text refer to page numbers of this issue.

suggests the study of something caged, waiting to disclose itself to anyone who has wit and method. Transforming "knowledge" into "knowing" puts the emphasis not on the deer to be categorized but on the chase, and the deer are not in an enclosed park but in the forest at large. Perhaps looking at theoretical propositions as though they were products to inventory has some value in expressing one's tentative understanding of the state of the art, but seeing the enterprise principally as a continual process is probably more useful.

Participants in the symposium either tacitly or explicitly endorsed the idea of research as process. Scheidel, for example, distinguished between "science as product and science as process," preferred the latter interpretation, and saw "scientific research as activity rather than primarily an accomplishment" (23).

Seeing research as process has at least two implications sometimes undervalued. *First, as process, research has no single starting point.* Some writers in the symposium proposed one activity as *the* starting point. Scheidel, in urging a practical approach, can be interpeted as arguing that its starting point ought to be the question to be answered. He contended, "The research question is all important, because it . . . provides the direction and the structure for the observations and conceptualization necessary to answer it" (26). Even though Scheidel identified the three phases in his analysis of the research process as "circular and interactive," his establishment of them in sequential form may be construed as implying a researcher should begin with the problem, with the "careful *observation*, *analysis*, and *description* of the phenomenon in which we are interested" (24).[2]

On the other hand, Delia and Grossberg advocated "a sound theoretic orientation" as "the starting point" (38). They characterized a theoretic orientation as "some interpretive understanding of persons, communications, and human social life." Their apparent sequencing of three general directives (38-42) also may be taken as implying the research process has a beginning. If I am right in believing Delia and Grossberg's interpretive understandings may be regarded more appropriately as philosophical assumptions than as a theoretic orientation, then they may be construed as urging philosophical assumptions as the starting point for research in communication.

In contending that "contemporary research in communication proceeds from a primarily empirical orientation" (9), Fisher identified empirical data as "the starting point" for much current research and disapproved of this state of affairs. He saw consistency between empirical data and a conceptual/theoretical rationale as the primary criterion to judge the admissibility of evidence, but he did not identify any single starting point as optimal. Perhaps

[2] Leonard C. Hawes has argued also that a study of the process of communication should "begin at the beginning," with "systematically observing, describing, and interpreting all manner of human communicative activity": "Alternative Theoretical Bases: Toward a Presuppositional Critique," *Communication Quarterly*, 25 (1977), 68, 64.

as an advocate of General Systems Theory, Fisher might concur with the claim I shall now advance.

If conducting research in communication is truly a process, no one starting point should preclude others. Such a process includes many activities: determining one's philosophical assumptions, committing oneself to a repertoire of theoretical constructs, interesting oneself in a particular research problem, conceiving a methodological design, knowing a battery of techniques for collecting and analyzing data, utilizing the research experience of others, respecting the integrity of communicative experiences of genuine persons participating in actual social settings, and reporting one's understandings to others. Each of these activities can serve as *one* perspective on the process, as *one* starting point for generating research projects. The first implication of the idea of research as process, therefore, is that no one activity "starts" or "ends" the attempt to gain a better understanding of persons communicating.

A second implication is that each of the activities enumerated above are interrelated. All participants recognized a close connection between the collection of data and one's tacit or explicit theoretical orientation. The very title of the symposium assumed such an interaction. As noted above, Fisher urged the consistency of evidence with the conceptual/theoretical rationale guiding the study as the most significant criterion for judging the admissibility of evidence. Grossberg and Delia criticized Scheidel for seeing "the observational-descriptive phase of science as distinct from the theoretical-explanatory phase" (53). Although Liska and Cronkhite called attention to Scheidel's characterization of his three phases as interdependent, circular, overlapping, and interactive (61), one may interpret his discussion of some of them as implying a distinction between observational and theoretical statements.[3]

Throughout their two essays, Delia and Grossberg placed a strong emphasis on the interrelationships between data representing the "meaningful character" of communicative experience "within everyday human life" (39) and the ontological question of "what it means to be human" (35). In addition they appeared to go beyond other writers by recognizing relationships among some of the other activities I have noted above. For example, they said, "without a theory defining for us the kind of data to collect, the methods to be used in appropriately securing that data, and so forth, the research enter-

[3] Scheidel's own philosophical position is a matter of controversy. Grossberg and Delia called it logical empiricism as toned down by a Popperian falsification doctrine (53). Liska and Cronkhite disagreed, claiming Scheidel's position is difficult to discern; but their arguments are unconvincing. To argue that Scheidel is not a logical empiricist because his views are not as "narrow" as those of A. J. Ayer (60-61) does not respond to Grossberg and Delia's claim that Scheidel appears to follow Karl Popper's principle of falsification, a significant departure from Ayer's principle of verifiability. Furthermore, to say Scheidel regards his three phases as interdependent and overlapping is not the same thing as saying that theory and phenomena are so inseparable that one cannot look at phenomena in a theory-free way. Scheidel appeared not to advocate such an inseparability when he stated, "We must know subject-matter prior to theory" (23).

prise becomes hopelessly undirected and its results without meaning and coherence" (38).

If research is a process, its every activity is related to *every* other activity. Persons who choose a philosophical perspective on the process as primary, for example, should make their epistemological-ontological assumptions consistent with the theoretical constructs treated in the research, with the kinds of questions asked, with the methodological design selected, with the kinds of experiences or behaviors used as data, with the techniques for collecting and analyzing the data, and with the styles and strategies for reporting the results. In addition, each activity should relate cogently with one another. To the extent possible, all of the interrelationships of all activities should constitute a consistent and coherent process as a whole.

II

Philosophical assumptions serve as *one* perspective on the research process. Since no one can see the whole of any process from every conceivable vantage point all at once, a person must pick some perspective. I choose a philosophical one because it is fundamental and often underemphasized.

Some writers in the symposium appeared to undervalue its importance. Scheidel concluded flatly, "Our problems with research evidence are more practical than they are philosophical" (30). Although Fisher stressed the role of a conceptual/theoretical rationale and discussed how four epistemological perspectives influence whether perceptual or behavioral data are to be collected in a research project, he did not broaden the role of philosophy beyond epistemology nor relate it to other activities in the process.

Throughout both of their essays, Cronkhite and Liska usefully distinguished between explanatory and epistemological metaphors, defining the latter as "a set of assumptions about how one 'knows' the world" (5). In their second essay they weakened the distinction by calling both metaphors "theories" (57). Early in that essay they criticized Delia and Grossberg for defending "what appears to be their basic assumptions" (57). Later, however, they entitled a section of the essay "Criteria for Epistemological Validation," in which they advocated "choosing among *epistemological* theories depending upon the problem at hand" (64). Although consistency between one's philosophical assumptions and the research problem at hand is important, one wonders if one can choose one's "basic assumptions" at will and whether philosophical assumptions ought not be consistent with *every* activity in a research project.

Delia and Grossberg raise philosophical questions about research throughout both of their essays. Although their apparent vacillation between the terms "philosophical anthropology" and "theoretic orientation" is sometimes confusing, for the most part they tend to use both terms to refer to epistemo-

logical and ontological questions about the nature of knowing and the nature of communicative "reality." They related their philosophical concerns to the nature and source of experiential data, to the techniques for collecting and analyzing the data, to theoretical constructs, and to the task of communicating one's discoveries to a specific scholarly community.

Although the title of the symposium points directly to epistemological questions, researchers in communication ought not ignore such other philosophical concerns as how they construe the nature of communicative reality (ontology) and how they choose some values as more important than others (axiology). In addition, philosophical assumptions should influence *every* decision concerning *every* activity in the research process—not merely criteria for the admissibility of evidence. For these reasons I join Grossberg and Delia in their agreement with Maurice Natanson when he stated, "Philosophy is inescapable for the social scientist who seeks clarity and rigor in his [or her] work, who takes the term 'discipline' seriously" (54).[4]

III

In their second essay, Grossberg and Delia discussed two general philosophical views of science: logical empiricism and interpretive stances. Their earlier essay, following John Wild, identified three general positions: the empiricist's idea of fact, the idealist's notion of essence, and the interpreter's recognition of an interplay between "fact" and "essence" (33). Fisher deplored much contemporary research's foundation on *empirical* assumptions. No participant in the symposium discussed the *idealist's* position; in our discipline that stance appears to be a null category. Only Delia and Grossberg explicitly endorse one of the three positions; they favor *interpretive* views.

For this they are criticized in two esasys. They may have invited these complaints by failing to make clear they were advocating one of three general philosophical positions, not one particular theoretic orientation. When they argued "for interpretive theories of communication" (35), they probably did not intend to claim the "correctness" of one "narrow" theoretic orientation. At least I read them as illustrating the utility of interpretive philosophical stances by reference to one theoretical prospectus.

Fisher criticized Delia and Grossberg for imposing on readers "advocacy of a particular theoretical orientation" and for assuming implicitly "the existence of some 'right' or 'correct' theory" (45). If the authors intended "theoretical orientation" to be consonant with Scheidel's definition of a theory, I concur with Fisher's criticism. But if they wanted a reader to understand the term as a set of philosophical assumptions, then they may be construed as

[4] "Alfred Schutz on Social Reality and Social Science," in *Phenomenology and Social Reality*, ed. Maurice Natanson (The Hague: Martinus Nijihoff, 1970), p. 101.

claiming interpretive stances as a meritorious base for research in human communication.

If theoretical orientation is taken as "explanatory," Liska and Cronkhite properly share Fisher's concern for Delia and Grossberg's advocacy of one "correct" theory. If it is construed as "epistemological," however, their criticism is questionable. They charge Delia and Grossberg with proposing "an extremely narrow definition of acceptable evidence" by endorsing "the assumptions of phenomenology as modified by Schutz' philosophical anthropology" (59), or of "an approach derived from phenomenology, i.e., constructivism, interactionism, as the epistemological perspective" (61).[5]

I see good grounds for believing Delia and Grossberg present their interpretive account of human communication not as one narrow philosophical position that mandates acceptance, but as a very wide range of views. I construe their philosophical position as affirming "a unity of person and world, of knower and known" (32), as saying "pure facts" do not exist and interpretations of facts by communicators (and presumably by researchers as well) are open to argument.

One basis for this reading of their essays is their recognition that "multiple perspectives are a requirement for the social sciences because only such a view respects the ambiguity of human experience" (34). Consistently they referred to interpretive stances in the plural form. They conceded that they themselves have "personal disagreements over which interpretive stance provides the most adequate grounding for the human sciences" (35). They even itemized some of the stances to be accommodated by an interpretive philosophical position, including "the Chicago school of symbolic interactionism, George Kelly's cognitive theory of personality, Heinz Werner's organismic developmental perspective, Raymond Williams' Marxist cultural theory, Alfred Schutz's phenomenological social theory, Aaron Cicourel's ethnomethodology, and so forth" (38). Taken as one of three broad philosophical options (along with logical empiricism and idealism), interpretive views are not necessarily derived from phenomenology; and they are very far from narrow.

What the various interpretive views have in common are an ontological concern for the person as an active and creative being, an epistemological commitment to pursuing knowledge about human communication as problematic, an axiological recognition of persons as choosers among values, and a methodological emphasis on the role of argument. Each assumption has implications for decision making in the process of research.

First, persons who take various interpretive stances share an ontological

[5] Although their quotations from Adolf Reinach, Peter L. Berger and Thomas Luckmann, and George Kelly may not reveal it, phenomenology itself is not a singular or narrow philosophical position. The views of Husserl, Heidegger, Merleau-Ponty, and Schutz—and the modifications of those views by contemporary writers—are significantly divergent. Most phenomenologists, though not all, take interpretive stances.

assumption of persons as active and creative beings in process. At a general level, every writer in the symposium, to one degree or another, concurred with this presupposition. Scheidel concluded his "Afterthoughts" with this statement, "The self plays a more significant role in the process of scientific inquiry than we usually acknowledge" (52). Liska and Cronkhite observed, "Scientific inquiry is a human enterprise" (63). Fisher described an interactional perspective on communication, one he espouses, as an "humanistic approach to communicative events" (14). Delia and Grossberg placed considerable emphasis on the researcher's understanding that their subjects are "acting, feeling, thinking individuals engaged in shaping their own understandings and utterances and those of their fellows within a reciprocal, ongoing process of interpretation" (37); and they recognized "inescapable personal commitments [of scientists] in the conduct of inquiry" (55).

This interpretive ontological conviction should permeate every activity of the research process. Conceiving, designing, conducting, and reporting research is no less a personal experience than are other communicative situations. Like other instances of communication, doing research in the process of communication itself implies an interaction among researchers, subjects, and consumers of research reports—all of whom participate in the process as *persons.*

Second, interpreters share an epistemological assumption of the world of communication as complex, ambiguous, and uncertain. This conviction denies a positivistic reliance on the interaction of "pure facts" and "pure logic" and also an idealistic faith in forms or essences. It rejects a dependence on absolutely objective or absolutely subjective data in favor of intersubjective tests of evidence (and other research activities) *in context.* It presumes that every activity in the research process is a matter of interpretation, that researchers argue their way to choices simultaneously to satisfy their own critical judgment and to convince the community of scholars with whom they argue.

Third, interpreters share the axiological assumption of a person as one who attaches values and makes choices about each activity in the research process. One of the controversies in the symposium illustrates the importance of this axiological commitment. Delia and Grossberg admonished researchers not to "impose analytic techniques with their own . . . ordering principles" but to secure "data in a form retaining the subjects' own structuring of their personal and social meanings" (39). Fisher responded, "The social actor is a biased source precisely because he/she did live those experiences. The observer is more objective and impartial." He concluded, "If the observer is the ventriloquist and the subject the dummy, then the converse is that the observer is the dummy to the subjects' ventriloquism" (47).

But interpretive philosophers see subjects and researchers as neither ventriloquists nor dummies. All persons potentially, as Delia and Grossberg

maintained, are "agents of action, not mere responders to events," and communication is "an emergent, creative activity through which human social reality is constantly being recreated, affirmed, repaired, and changed" (36). Researchers and subjects are choosers among values as well. I cannot conceive the possibility of any research design that makes the biases (values) of subjects and researchers altogether nonproblematic; but under the inevitable constraints within the context of a particular project, researchers choose and argue as well as they can.

One hazard in overlooking axiological issues is that a person may ignore ethical considerations. None of the criteria proposed by any of the participants spoke directly to a need for ethical standards in judging the admissibility of evidence. Evaluating evidence or judging any other activity in the research process, however, should take into account whether a researcher has met scrupulously his or her own personal standards and those of the community of scholars to whom a research report has been communicated.

Fourth, interpreters are committed to a person-centered, context-dependent, uncertainty-regulating method of argument. Cronkhite and Liska introduced the symposium with an emphasis on the role of argument in research (3-5). Other participants appeared to concur. They raised two significant issues related to the role of argument in an interpretive view of research.

A researcher-theorist must be concerned with the nature of the community within which he or she works. Grossberg and Delia claimed, "The choices among competing theoretical paradigms are made within the context of larger community decisions, [and] the evidence upon which such choices inevitable [sic] turn embody both the standards of rationality historically developed as part of the meaning of science in our culture and the particular tests and criteria of the specific scholarly community" (55). If the criteria of a *specific* scholarly community are important, one wonders how many or how few persons constitute it.

I interpret Liska and Cronkhite as implying (63-64) that persons who share epistemological assumptions can compare, evaluate, and argue about conflicting theories. I fully agree, and I would add that they can argue also about every activity in the research process. I agree also with them when they say if competing theorizing (and other research activities?) "are derived from different epistemologies, there is no basis for direct comparisons" (64). Arguing is not likely to be productive when researchers share too few philosophical assumptions.

Persons who share primary philosophical, theoretical, methodological, and technical assumptions—together with a mutual interest in a field of inquiry—may comprise a relatively small group of persons who can contribute greatly to one another's understandings. Persons who share some assumptions and some interests will comprise a larger group of persons who can

argue with some profit. Persons who differ radically about philosophical assumptions undergirding research activities will have little they can argue about.

The second issue involves the kind of criteria persons may use to evaluate research evidence (or any other activity of the process). Liska and Cronkhite concluded that Fisher's four criteria, Scheidel's criteria for the three phases of research, and their revision of the two criteria Delia and Grossberg advanced provide "a list that is commendably comprehensive, general, and useful" (65). If one construes research as field invariant and seeks only general guidelines, the list may be useful. But if one views research as context-dependent, sees every activity as related to every other activity, and rejects logical empiricism and idealism in favor of one of the interpretive philosophical positions, then the advice of Delia and Grossberg to "be smart and do good work" (38) may be as much to the point. Interpretive judgments in a complex and ambiguous world are not easy.

A critical reader of research will perceive claims as convincing only if he or she can understand how philosophical assumptions, theoretical constructs, a methodological design, sources of data, techniques of collecting and analyzing data, and the style and strategy of reporting the research—*all in the context of a particular project*—fit together appropriately. Philosophical assumptions will loom large in determining whether a researcher's argument has a stronger rationale than those of competing claims for the community of scholars to whom it is addressed.

IV

The symposium is stimulating. My responses are these: Research is a process with no single starting point; its activities in any particular project must be interrelated to form a coherent whole. The philosophical assumptions of researchers and readers of research reports are especially significant in relation to every decision of every research activity; and ontological, axiological, and methodological presuppositions need to be considered along with the epistemological issues raised in the symposium. Interpretive philosophical positions should not be construed as narrow but as a wide range of stances conjoined by several commitments: a person-centered ontology, an uncertainty-regulating epistemology, a value-choosing axiology, and a context-dependent view of argument at the heart of methodology.

The Reflexivity of Communication Research

LEONARD C. HAWES*

THE WINTER 1977 ISSUE of *WJSC* addressed itself to the following question: "What criteria should be used to judge the admissibility of evidence to support theoretical propositions regarding communication research?"[1] By way of offering yet another answer, let me suggest that even if practicing researchers could agree on admissibility criteria as written down in some essay, applying the criteria in the actual doing of research would be problematic in every instance. Such is certainly the case in law, a social institution priding itself in evidenciary criteria. Competent attorneys, trained to know and use admissibility criteria, prepare cases using evidence that other attorneys would disagree with. Judges rule on the admissibility question each time it surfaces in court and those rulings can be, and are, appealed to other courts and judges. Social science unfolds similarly; competent social scientists conduct and report research using evidence with which other social scientists disagree. Referees and editors pass judgment on the admissibility issue. But each judgment can be appealed by submitting the report to other journals and their editorial boards. Criteria symposium contributors formulate will become socially meaningful only in the practical accomplishments of doing communication research.

Social science, like law, encompasses a broad ensemble of practices and procedures in which members engage to accomplish practical activities recognizable as scientific or legal. If evidence is defined as, "an appearance from which inferences may be drawn; an indication, mark, sign, token, trace,"[2] each of the ensemble of practices, which together constitute the doing of communication research, relies on its own evidence. To assume there is only one dimension of evidence and that specifiable criteria can be used objectively to screen out inadmissible evidence is to oversimplify grossly the doing communication research.

If one accepts this preface, at least for the argumentative purposes of this symposium, then one way to proceed is to examine communication research

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1 Gary Cronkhite and Jo Liska (moderators), "What criteria should be used to judge the admissibility of evidence to support theoretical propositions regarding communication research?", *Western Journal of Speech Communication*, 41 (1977), 1-68.

2 Gary Cronkhite and Jo Liska, "Introduction," *Western Journal of Speech Communication*, 41 (1977), 7.

as a practical human accomplishment, much the same way as selling insurance, conducting an interview, buying a car, getting married, or taking a vacation are practical human accomplishments. Each of these activities is, in large part, a communicative accomplishment; a variety of communicative activities must be performed appropriately, and each in an appropriate sequence, for the accomplishment to be recognizable and socially practical. Some of these communicative activities involve the telling of stories. The researcher tells one story of his/her project to prospective collaborators, another story to prospective funding agencies, a third story to subjects and participants, a fourth story to graduate assistants, a fifth story to convention critics, a sixth story to journal editors and referees, and so forth.

Social scientists, like lawyers, are trained and revered story tellers in Western society. The social scientist is trained to notice particular appearances and to make inferences from those appearances. The inferences, when stated in writing or talking, become propositions. Inferred from particular appearances, these propositions are then systematically related into larger constellations of propositions. The clusters of propositions become detailed stories or interpretations in light of which individual propositions become plausible and understandable. To relate systematically the individual propositions, the social scientist tells a story as much in keeping as possible with the originally observed appearances. The story is called typically a theory and the individual propositions comprising the story are called theoretical propositions.[3] To the extent that communication researchers notice appearances, formulate propositions, and tell stories consisting of these propositions, some criteria for the admissibility of evidence ought to be grounded in the adequacy with which the stories are told.

Each story, to be minimally acceptable, must be told according to a logic of justification.[4] For any practical accomplishment, logics of justification can be formulated; communication research is no exception.[5] The logics of justification are communicatively constituted inasmuch as a justification for any activity does not exist as a social phenomenon until someone calls for its articulation. From this perspective, the symposium is a search for criteria to assess justificatory logics of communication research. In responding, the contributors are bringing those logics into social being communicatively in the form of essays. In short, I am arguing that the justificatory logics of and

[3] For a decidedly literary perspective of stories and their criteria, see the introductory materials of Mark Schorer, ed., *The Story: A Critical Anthology* (Englewood Cliffs, N.J.: Prentice-Hall, 1950); for a more sociological/phenomenological account of theory-building as constructing life stories, see: Alan F. Blum, "Theorizing," in *Understanding Everyday Life*, ed., Jack D. Douglas (Chicago: Aldine, 1970), pp. 305-19.

[4] Richard S. Rudner, *Philosophy of Social Science* (Englewood Cliffs, N.J.: Prentice-Hall, 1966), p. 5.

[5] Marvin B. Scott and Stanford M. Lyman, "Accounts," in *Symbolic Interaction*, 2nd ed., eds., Jerome G. Manis and Bernard N. Miltzer (Boston: Allyn and Bacon, 1972), pp. 404-29.

for communication research are themselves communicative. The criteria for assessing evidence in communication research ought, therefore, to focus on their adequacy in the communicative accomplishment of telling a particular story.

If communication research is a practical human accomplishment, and that accomplishment is the telling of stories or myths that increase the human specie's chances of survival,[6] we may begin by examining three relevant presuppositions of justificatory logics for social scientific stories.[7] First, it must be supposed that people's actions can be separated from the particular circumstances in which those actions are performed. If this presupposition is not granted, a researcher conducting an experiment on group discussion, for example, would be constrained to present the subjects' particular utterances in terms of how those utterances accomplished that particular discussion, which in turn accomplished that particular experiment. What subjects say and the conditions under which they make those statements could not be separated. The researcher would have to provide a detailed description of a particular and unique occurrence. If an attitude change researcher has subjects fill out a set of semantic differential scales, the research report would consist of a detailed description of the circumstances under certain people made marks on certain sheets of paper. The first presupposition, then, is that people and their activities can be separated analytically. This move allows for the possibility of data.

A second presupposition of social science stories is that the detached activities can be transformed into a variety of things. The hypothetical group discussion the researcher is interested in transforming the utterances of individual group members into one or more of a variety of catgories. The objective is to work with categories and not particular utterances. To change a particular utterance, such as "but I don't understand, uh," into a category, such as *Requests Clarification*, allows the researcher to reduce the variability of the original activity to more manageable proportions; the particular instance is transformed into a general class. These classes are now the data. If the second presupposition is denied, the researcher would be confronted with audio tapes, transcripts, or individual sheets of pencilled markings and would be frustrated in the attempt to tell a manageable, compact story. Transforming detached activity (data) into general classes allows for the possibility of hypothetical constructs.

The third presupposition is that the structures organizing data as hypothetical constructs can be treated as distinct entities to which agency, or pos-

[6] Jo Liska and Gary Cronkhite, "Epilogue for Apologia: On the Convergent Validation of Epistemologies," *Western Journal of Speech Communication*, 41 (1977), 62.

[7] These presuppositions are borrowed from Dorothy Smith, who in turn, borrowed them from Marx and Engels; see: Dorothy Smith, "Theorizing as Ideology," in *Ethnomethodology*, ed., Ray Turner (Baltimore: Penguin Books, 1974), pp. 41-4.

sibly causality, can be attributed. The researcher attributes hypothetical constructs to people and/or collectivities who are now said to *re-present* them in their subsequent activities. The constructs, now attributed to typical people, are said to be observable and measurable as entities influencing or causing that which is observed subsequently. The group process researcher identifies a structure of utterances as accomplishing leadership; leadership is then attributed to typical group members who re-present leadership to researchers trained to notice certain appearances from which leadership may be inferred. That construct can now be said to influence other activities in specifiable ways. The attitude change researcher factor analyzes the semantic differential scales and the factors are attributed to persons doing persuasion; the structure of factors is said to be re-presented in subsequently observed attempts to change attitudes. Attributing hypothetical constructs to individuals and collectivities allows for the possibility of theoretical propositions.

Before the general story or theory can be told, three activities must be accomplished. First, situated activity must be detached from its circumstances of occurrence, thus producing data. Second, data must be transformed into general classes which can be used as hypothetical constructs. And third, hypothetical constructs must be attributed to typical actors who re-present them to researchers trained to translate their observations into theoretical propositions. Finally, the researcher constructs a general story by weaving together the theoretical propositions into a plausible whole.

Telling a story in keeping with the presuppositions of social science, the story teller is engaged in a persuasively practical activity.[8] Other story tellers must grant the story at least the status of plausibility. To accomplish the appearance of plausibility, and thereby persuade skeptical colleagues, the story teller must provide sufficient detail not only in the telling of the general story itself, but also in the internal stories about how the constitutive moves were accomplished. Social scientists are supposed to be conservative story tellers and skeptical story consumers. The concern of this symposium is in keeping with that skepticism; not just any kind of evidence can be used in a story about human communication. Social scientists demand to know of one another how people and their situated activities are separated, how separated activities subsequently are organized, and how the organized activities are given back to typical actors in the form of attributed constructs.

Another way of formulating the preceding material is by arguing that in communication research, communication is both the *topic* of research as well as the *resource* for conducting the research.[9] The implications are fascinat-

[8] George Steiner, "The Claims of Theory," *After Babel: Aspects of Language and Translation* (London: Oxford Univ. Press, 1975), pp. 236-95.

[9] This is a standard ethnomethodological formulation in the reflexivity of social science research. See: David Silverman, "Methodology and Meaning," in *New Directions in Sociological Theory*, eds. Paul Filmer, Michael Phillipson, David Silverman, and

ing. Communication research is simultaneously referential and reflexive; our work refers to entities outside the work itself, and at the same time the work is the embodiment of much of what is being studied. Perhaps an example might help. An attitude change researcher studying persuasibility asks thet following question: "are women with restricted speaking codes more persuasible than women with elaborated speaking codes when presented with a persuasive message delivered in an elaborated speaking code?" In actually doing the study, the researcher must use the very phenomenon s/he purports to study; the researcher at some time must persuade the sample of women to participate and cooperate in the study. The researcher's persuasive story is delivered in a speaking code more or less elaborated. It would be highly unlikely, however, if our hypothetical researcher saw fit to comment on this inherent reflexivity.

In any study involving other humans, a researcher must describe the study in such a fashion that it makes sense to those being asked to participate. In deception studies particularly, attention is given to an account of the study that, although a lie, is a story participants will find plausible. Yet in very few studies are we told in any detail about such communicative accomplishments. We are told what class subjects were taking and that they participated. We are not told how their participation was obtained; what persuasive strategies were used, to what extent credibility was manipulated, were threats or reprisals implicated, was the persuasive story changed over repeated tellings, or what level of lexical complexity was used in telling the stories.

Frequently in attitude change studies, a recorded message is used as one independent variable. How often are we given that message? How often are we told in any detail how the message was written and how its speaker was selected? How often are we given any description of the style in which the message was performed (e.g., stress, tone, pitch, rate, volume)? In the studies I read and review, the answer is: not very often. Yet this message, of which we are told so little, is an entity to which powers of influence and sometimes causality are attributed by way of explanation.

In interaction analysis research, little is reported about how the researcher formulated the coding categories, the communicative activities engaged in for training coders, the communicative activities among coders during training and "real" coding, how coding acceptability was negotiated, and so forth. These communicative activities are glossed by inter- and intra-coder reliability coefficients and a sample definition of the categories. The latter material may be useful and necessary summary information but without the former, its adequacy is difficult, if not impossible, to assess. Yet we frequently accept

David Walsh (Cambridge, M.I.T. Press, 1972), pp. 183-200. For an earlier more specific and detailed critique of social science methods based on the over-looked topic/resource distinction, see: Aaron V. Cicourel, *Method and Measurement in Sociology* (New York: Free Press, 1964).

coded talk as data knowing little else than what the categories are, how they are defined, and reliability coefficients.

In field interview research, interviewers largely ignore the communicative activity of eliciting answers to questions. The concern is to code interviewee responses; how those answers are obtained communicatively is not considered worthy of reporting. Yet an interview is a rather complex and largely improvised communicative activity. Even if the interviewer's questions are pre-specified, when and how to ask the questions must be improvised on the spot; the interviewer's communicative competence is taken for granted. We are told sometimes, that the interviewer was trained, and the duration of training. But the communicative activity producing what becomes data is overlooked.

In a delightful contribution to the "My Turn" section of the 25 July 1977 issue of *Newsweek*, Arlen J. Hansen, an English teacher at the University of the Pacific, writes of this same lack of reflexive awareness in telling stories. One of his anecdotes is worth repeating here not only because it is itself a good story, but also because it addresses itself directly to the point I am developing. Hansen writes:

> Last spring at exam time, one of my best students told me she would have to miss a few days of class because her grandmother had died. Now, students have been killing off their grandmothers at exam time for decades. I've had students do away with as many as four grandmothers in one year. So, I asked this student how her grandmother had died. 'Well, uh, she was in a car accident,' the young woman answered. 'Where?' She looked blankly at me—'On 101. Near Bakersfield.' Since Bakersfield is not on U. S. Highway 101, I tried to help out: 'You mean Highway 99?' 'Yes,' came the answer, followed quickly by 'and my brother was with her.' The plot was thickening. 'How did it happen? I mean, did your brother get hurt too?' The student brushed some lint off her shoulder. 'He's in the hospital.' I waited, and eventually she continued. 'My father had a heart attack last week.' Morbidly capitalizing on the opportunity, I pressed on. 'How's he doing?' 'OK.' 'I'll bet the accident gave him a relapse.' 'I suppose,' she said.
>
> I ended the interrogation. The plot had not thickened at all. It had simply been compounded. She worked by adding on; more is better. The student had killed her grandmother, seriously injured her brother and inflicted a heart attack upon her father. It was ruthless. Cold-blooded. I was reluctant to ask for more, out of fear that she might do damage to her mother and sister as well.
>
> The poor student simply did not know how to storify. She had no sense for detail. There was no gore, no broken bones, lacerations, shattered glass, screams of terror and pain and no complications. No imagination.

Hansen's student was not aware of the reflexive irony of the situation; she is telling an English teacher a story which displays none of the principles

she supposedly learned to become a good English student. She does not recognize that to make an excuse is to tell a story; both are practical activities inasmuch as it is in the story-teller's best interests to have the chosen audience believe the story. Perhaps the student is not accustomed to critical audiences for her everyday stories.

The parallel between the student's excuse and many of our research reports, however, is apparent; neither shows much recognition of or appreciation for the reflexive nature of the respective activities. As a result, neither sees the need for providing much detail; the student wants to be excused and the researcher wants to report some findings. Neither wants to tell an elaborate and interwoven set of short stories; the student and teacher have limited time and the researcher and editor have limited space. Consequently, many reports of communication research are as unconvincing as the student's excuse. Both have more than their fair share of naivete.

I am not arguing that the presuppositions of social science discussed earlier are inadequate or inappropriate. As Delia and Grossberg stipulate in their essay, to do social science at all is to rely on certain abstractive principles.[10] I am arguing that the abstractions from uniquely situated human activity to theoretical propositions and eventual theories are communicative accomplishments, and that we devote insufficient attention to reporting the details of these accomplishments and their impact on validity and reliability claims.

Is it realistic to expect any given study to provide a detailed reflexive account of its own accomplishment? Probably not; but that is not the intended implication of my argument. I am maintaining that if most communication research procedures are themselves communicative activities, the logic of justification constituting the methodology of our research ought be firmly grounded in our knowledge of those communicative activities. Framed another way, the rationale we offer not just for *what* we study but *how* we study ought to be constructed from knowledge claims about communication, themselves established social scientifically with critical reflexive awareness.

To conduct research in accord with this perspective requires that researchers recognize research to be a complex and extended form of practical social activity.[11] Any social activity is accomplished, in large measure, in and

[10] Jesse G. Delia and Lawrence Grossberg, "Interpretation and Evidence," *Western Journal of Speech Communication*, 41 (1977), 33-4.

[11] I am arguing that we approach and engage in communication research much the same way Alan Blum approaches "theorizing" and Robert Pirsig approaches the art of motorcycle maintenance. Pirsig develops two types of people; those who like to ride motorcycles as long as they remain unproblematic and those who are as intrigued with how the bike works as they are with the riding experience. The latter person, unlike Hansen's English student, sees little distinction between riding and maintaining because of the reflexive curiosity and the attention of detail this rider brings to the motorcycle. Being able "to get into" bikes, literally and figuratively, is the reflexive curiosity I am trying to explicate. The anologue is that we get intrigued with the reflexivity of com-

through an ensemble of communicative activities. We claim communication as the province of our inquiry. Thus, what we study and how we study stand in reflexive relation to one another. I am proposing only that we recognize this inherent reflexive relationship and exploit it to the benefit of our research.

In research reports, procedures and methods sections could footnote our own empirical research as a rationale for how subjects were obtained, coders trained, scales developed, field co-operation secured, stimulus messages written and recorded, and so forth. What do we know about social influence, persuasion techniques, effects of status on perceived credibility, situational demand characteristics that might lend insight into the communicative activity of a researcher "asking" students to participate in a study? What are the implications for arguing the validity of the resulting data and the transformational uses of such data? Extant communication research answers these questions, at least in part. Those knowledge claims ought to be acknowledged not merely on a common-sense level, like Hansen's student and her common-sense knowledge of excuses, but also on a technical level in both designing and reporting the research.

What do we know about the oral performance of literature, dramatic interpretation, the structure of everyday conversation, and speech styles that would lend insight into how pre-recorded persuasive messages might be constructed by researchers and heard by subjects? What notational conventions might be used in displaying the style of persuasive messages? What do we know about group discussion and decision-making that would lend insight into how a group of coders might formulate a set of categories for coding verbal utterances? What are the implications of levelling and sharpening in group discussion on the resultant set of categories and how utterances are transformed into category classes?

I am arguing that these questions be asked of how we accomplish our research. Becoming sensitive to the reflexivity of our work ultimately can lead to more critical story telling; the justificatory logic reflects the inherent reflexive nature of the activity affording the myths produced greater epistemological as well as existential validity. Asking communicative questions about the accomplishment of communication research generates additional communicative questions worthy of additional research activity. Such a perspective ensures the compatability of epistemological and explanatory metaphors, whatever their specific content may be. If, as Cronkhite and Liska contend, "the central question of this symposium can be viewed as asking what epistemological metaphors are especially important in the study of communication,"[12] one defensible answer is that epistemics are communica-

munication research much as Pirsig is intrigued with the reflexivity of motorcycle maintenance. See: Robert M. Pirsig, *Zen and the Art of Motorcycle Maintenance* (Toronto: Bantam Books, 1974).

[12] Cronkhite and Liska, "Introduction," p. 5.

tively accomplished and revised in and through the social activity known as communicative research. To exploit the reflexivity of our work is to ensure the integrity of the dialectic between epistemological and explanatory metaphors.

The Wartofsky material Liska and Cronkhite use in the "Epilogue . . ." is the same argument formulated at a more macro-science level:

> The epistemological constraints on science are social, and the social constraints on science are epistemological; and both our ways of knowing or our modes of scientific inquiry and the institutional forms and social uses of this knowledge are historical. Ways of knowing change with social practice and in turn, social practice and social structure changes with shifts, revolutions or transformations of our ways of knowing.[13]

I do not mean to imply that Liska and Cronkhite would agree at all with the particular perspective I have articulated here. But their awareness of the reflexiveness of scientific inquiry is apparent particularly in their use of Wartofsky.

By way of drawing this discussion to a close and coming to specific grips with the question of the symposium, let me pose several questions which elaborate the reflexive dimension of communication research.

1. Does the research acknowledge its constitutive communicative elements?
2. Are communication knowledge claims used as evidence in the rationale for the methods, procedures, and techniques employed in the research?
3. Are communication knowledge claims used as evidence for demonstrating internal and external validity implications of the research?
4. Are communication knowledge claims used as evidence for arguing reliability implications?
5. Does the account of the methods and procedures of a study, separate from the study's acknowledged results or findings, point to additional communication research activity?

These criteria can be used to critique the very arguments producing them, inasmuch as the arguments are communicative activities recognizable as one form of a social science story.

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[13] M. W. Wartosfsky, "Social and Epistemological Constraints on Science," a paper presented at the American Association for the Advancement of Science, 1974, p. 1.

In Praise of Muddleheaded Anecdotalism

HERBERT W. SIMONS*

OF THE ESSAYS contributed to the 1977 *WJSC* symposium on admissable evidence, Thomas M. Scheidel's expresses views which conform most closely to the traditional model of the scientific process, what might be called "orthodox empiricism."[1] Scheidel conceives of the process of scientific inquiry as progressing from theory-development based on informal observation and creative invention to theory-testing based on carefully deduced hypotheses and rigorous tests of those hypotheses. He comes down hard on theories that are not "fully developed and tested to my satisfaction."[2] Consistent with what Jack D. Douglas has referred to as the ruling "hypothetico-deductive-statistical" paradigm of the social sciences,[3] Scheidel pictures well developed theories as capable not only of giving "rational coherence to a body of known empirical laws,"[4] but also of yielding clear and straightforward hypotheses which are confirmed by research evidence that he can "see, feel, hear, experience, and measure."[5] Presumably, as in the traditional view, these tests should permit the cumulative development of new laws, and further strengthen the theories capable of explaining them.

Like Scheidel, I too have been trained in the hypothetico-deductive-statistical approach, and I too appreciate its logic. Yet I must confess to a problem, one that may be shared by some readers. Put simply, the theory and research on communication that most closely conforms to the standards Scheidel and I have internalized frequently disappoints me, while the scholars I find most compelling often construct their theories rather loosely, and hang their propositional claims on the slenderest of empirical threads. I have registered my dissatisfactions with applications of orthodox empiricism to the area of persuasion in the other essay I have in this issue. Here I should like

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[1] Thomas M. Scheidel, "Evidence Varies with Phases of Inquiry," *Western Journal of Speech Communication*, 41 (1977), 20-31. Hans Reinchenbach offers a similar account of the stages of scientific inquiry. See: *Experience and Prediction* (Chicago: Univ. of Chicago Press, 1938). Orthodox empiricism is also well defended in Israel Scheffler's *Science and Subjectivity* (Indianapolis: Bobbs-Merrill, 1967).

[2] Scheidel, p. 30.

[3] Jack D. Douglas, "The Rhetoric of Science and the Origins of Statistical Social Thought: the Case of Durkheim's *Suicide*," in Edward A. Tiryakian, ed., *The Phenomenon of Sociology* (New York: Appleton-Century-Crofts, 1969), pp. 44-57.

[4] The quote is taken from Floyd Allport's definition of theory, cited approvingly by Scheidel, p. 23.

[5] Scheidel, p. 30.

to sing praises to the seemingly more fuzzy-minded counterparts of the orthodox empiricists, whom I shall call "muddleheaded anecdotalists." Unlike Scheidel, I am beginning to suspect that "good" anecdotalism has a justificatory logic of its own, and that it cannot be "reduced" to the language of orthodox empiricism without great loss.

Scheidel acknowledges that our most fertile theories are likely to be developed by persons with a broad, perceptive, exploratory cast of mind; those able to see the "big picture"; those willing, even, to utilize introspection, or other forms of tacit knowledge. He even admits to having "learned more about the speech communication process from the introspective accounts of certain psychiatrists than . . . from many tightly controlled experimental studies."[7] Scheidel's psychiatrists may not be my psychiatrists, and he and I would undoubtedly name other great influences besides, but I think I know what he means. Indeed, I am confident that many of my "good" anecdotalists would appear on his list of fertile thinkers. My own list is headed by Gregory Bateson and Kenneth Burke, but it also includes, among contemporary writers, such creative spirits as Erving Goffman, Jules Henry, R. D. Laing, Thomas Schelling, and Paul Watzlawick.[8]

Unfortunately, Scheidel makes no distinction between the "proof" requirements for a Kenneth Burke or one of the psychiatrists from whom he has learned so much, and more orthodox, empirically minded theorists such as Gerald Miller or B. F. Skinner. Rather, he suggests that all theories should ultimately be framed in reductionist terms that are capable of being operationalized. Presumably, even Burke's theories should be ground through the reductionist mill—reformulated in such a way as to permit empirical tests of his hypotheses. A Burkian theory might thus be rewritten to sound a bit more like Gerald Miller; and a Laingian formulation might be translated into the language of B. F. Skinner. From the tenor of Scheidel's comments about the necessity of rigorous tests of theoretical predictions as a precondition for assigning credence to a theory, one wonders in what sense he has "learned" from the types that populate my list, he ultimately indicates that they are only originators of ideas, unable to compel assent because they "argue from private facts."[9]

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[6] See: "The Rhetoric of Science and the Science of Rhetoric," pp. 37-43.

[7] Scheidel, p. 28.

[8] See, for example, Gregory Bateson, *Steps to an Ecology of Mind* (New York: Ballantine, 1972); Kenneth Burke, *The Rhetoric of Motives* (Englewood Cliffs, N.J.: Prentice-Hall, 1950); Erving Goffman, *Interaction Ritual* (Chicago: Aldine, 1967); Jules Henry, *Pathways to Madness* (New York: Vintage, 1973); R. D. Laing, *Self and Others* (Baltimore: Penguin, 1969); Thomas Schelling, *The Strategy of Conflict* (New York: Oxford Univ. Press, 1960); Paul Watzlawick, *How Real is Real?* (New York: Vintage, 1977). I am not suggesting these persons are the "best" anecdotalists; indeed, readers might well wish to suggest other such theorists whom I have either excluded or have not read. Cassirer, Edelman, Habermans, Langer, and McLuhan come to mind as examples.

[9] Scheidel, p. 29.

For me, the distinction between types of theories is all important. Were I to reconstruct Scheidel's model of the stages of scientific inquiry, I would place the Burkes and the Skinners of the world side by side (rather than classifying the Burkes as prescientific or pretheoretical), each functioning as observers, theorists, and "testers" of theory, albeit in fundamentally different ways. Contrary to Scheidel, I do not believe that the former, the anecdotalists, necessarily argue from "private facts," though I acknowledge that they do not offer "proofs" in the strict scientific sense of the term. Max Black may have exaggerated in his review of *The Grammer of Motives*, but I can see the point of his conclusion that "The vast rambling edifice of quasi-sociological and quasi-psychoanalytical speculation seems to rest on nothing more solid than a set of unexamined and uncriticized metaphysical assumptions."[10] William Rueckert, a major champion of Burke, has conceded that Burke "does not use evidence in a way that pleases rigorous and scientifically trained academicians."[11] Though not all of the writers on my list are as carefree about evidence as Burke is, none is an orthodox empiricist by any means.

THE ANALOGIC LANGUAGE OF ANECDOTALISM

Characterizing groups of scholars is tricky business. One can always find exceptions or overlaps among them. Nevertheless, it seems to be generally the case that anecdotalists think and communicate *analogically* rather than *digitally*. As a consequence, they tend to err in the direction of being muddleheaded, whereas orthodox empiricists tend to err in the direction of being simpleminded.

In his essay on "Communication as Bits and Metaphor," Jay Haley makes essentially this same point as applied to psychodynamic versus behavior modification therapists:

> Digital communication consists of that class of messages where each statement has a specific referent and only that referent. Something happens or it does not happen; there is *one* stimulus and *one* response. The use of digital language to describe human behavior appears most appropriate when the subject is the study of a human being dealing with the environment—when a person is building bridges. This language begins to be problematic when it is applied to human beings dealing with one another. From the digital view, the description of a man pounding a nail and the description of a man fighting with his wife should be synonymous descriptive problems. In both cases it would be a matter of phrasing the description in a precise language of single referents. Yet describing an interchange between husband and wife in digital language may leave out the essence of the interchange. The fight a married couple may have over who is to pick up whose

[10] His review is reprinted in William Rueckert (ed.), *Critical Responses to Kenneth Burke, 1924-1966* (Minneapolis: Univ. of Minnesota Press, 1969), pp. 166-69.
[11] Rueckert, p. 169.

socks have tended to mean in the context of the relationship.

When a message has multiple referents, it is no longer a 'bit' but is analogic, in that it deals with the resemblances of one thing to another. It is a language in which each message refers to a context of other messages. The analog can be expressed in a verbal statement, as in a simile or verbal metaphor. It can also be expressed in action—the showing of how something is by acting it out.

If there were a continuum from digital behavior to analogic, the problem could be more easily resolved. But there appears to be a discontinuous change from one style of communication to another. This change forces a dichotomy (italics mine).[12]

Haley goes on to point out how quite different psychodynamic schools of therapy utilize analogic modes of communication, while behavior modification approaches combine digital and analogic modes in therapy, and strictly digital modes in describing problems or evaluating outcomes of therapy. Comparisons between diagnoses or between claims of success are thus rendered extremely difficult. For example, the patient's visible symptoms as well as his/her statements about his/her symptoms may be seen by the psychodynamic therapist as metaphorical expressions of more complex pathologies. Description of dreams, free associations, and routine interactions with the therapist may all be examined for their metaphorical content. By contrast, the behavior modification therapist is more likely to treat the symptom itself as the pathology, and to consider the patient "cured" when, by various digital measures, it appears that the symptom has disappeared.[13]

Haley's observations apply to other anecdotalists as well. The persons on my list "muddle" their theories with statements about the contexts and relationships among messages. In place of single referents, they present us with levels and layers of meaning. In place of "either-or" thinking, they offer us "both-and" thinking. In place of discrete categories, or even linear dimensions, they offer us hierarchical orderings.

The seemingly disparate theories of Burke and Bateson are much alike in these respects. In Burke's theoretical framework, such dialectical terms as identification and division take on meaning in the context of their linguistic opposites, contain the seeds of their own negations, and provide the basis for arriving at "ultimate terms" and ordering principles. Bateson's ordering principles are meta-rules—rules about the rules people live by in ongoing relationships. For Bateson, as for Burke, humans are distinctive, not simply because they are symbol-using animals, but because they metacommunicate; that is, they symbolize about their symbols. In the confusions and inconsistences among levels, one finds the sources of the semantic and pragmatic paradoxes that bedevil us—the grounds for many of our most serious mis-

[12] Jay Haley, *Problem-Solving Therapy* (San Francisco: Jossey-Bass, 1976), pp. 83-4.

[13] Haley, pp. 81-99.

understandings, of our communicational pathologies, and of our existential despair. But in the relationship among levels of meaning, one also may unravel the mysteries of humor, play, art, and ritual, and discover ways out of the binds that imprison us.[14] Both Burke and Bateson are attuned to the symbolic content in forms of expression; to the way first-level messages are interpreted or disqualified at meta-levels of communication. In Burke's analysis of Gray's *Elegy*, for example, one finds shades of Bateson on the meta-communications of the schizophrenic child—both Gray and the child crying out in their own peculiar metaphors for recognition and approval.[15]

The anecdotalists on my list dance between fact and fiction; between the sciences and the arts. Goffman has more in common with Franz Kafka than with Talcott Parsons; Bateson finds more affinity with Blake than with Skinner. Those who would describe human relations "objectively," or who would "count" double-binds are ridiculed,[16] while play, humor, drama, poetry, and even myths and word-magic are taken quite seriously for the larger truths that they may contain. The overarching theoretical metaphor is often dramatistic: for Schelling, a game, for Goffman, front- and back-stage, for Burke, the ritual drama. Within these dramas, people both enact roles and *are* those roles, and they both shield and reveal themselves by the personas they display.

The concern for levels and layers of meaning has made possible much more sophisticated analyses of the forms of deception than is found in the attitude-change literature on sincere versus insincere speakers, forewarned versus unforewarned audiences, implicit versus explicit conclusion-drawing, and so on. Goffman is, of course, the master here,[17] but one also finds great profit in Schelling on "loss of control" techniques,[18] in Henry on sham (both black and white),[19] in Laing on mystification and collusion,[20] on misidentification techniques,[21] and in Watzlawick on misinformation and disinformation.[22] Yet these same writers are also cognizant of the means by which communication is used to form bonds with others. Central in all of their theories

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[14] These common characteristics of their theories are found in all their writings. Among persons in speech-communication, Lawrence Rosenfield, Laurie Hayes, and Thomas Frentz appear to have come closest to their orientation. See: *The Communicative Experience* (Boston: Allyn and Bacon, 1976). Though I have focused on contemporary writers, the debate is an historic one, often cast as a conflict between holistic and molecular orientations. See: Melvin H. Marx and William A. Hillix, *Systems and Theories in Psychology* (New York: McGraw-Hill, 1963).

[15] See: Burke's *Rhetoric of Motives*, pp. 124-5 and Bateson's *Steps to an Ecology of Mind*, pp. 201-27.

[16] See: Burke on Bentham, for example (pp. 98-9) and Bateson on criticisms of his double-bind theory (pp. 271-8).

[17] See Goffman's essay on "face-work" in *Interaction Ritual*.

[18] Schelling, pp. 21-52.

[19] Henry, pp. 181-6.

[20] See: Laing, Part Two, pp. 81-173.

[21] See: Burke on Ovid, for example, pp. 159-61.

[22] See: Watzlawick, pp. 45-140.

is a recognition of the mixed-motive nature of ongoing relationships, of our simultaneous need to compete and to cooperate with others. In Burke's dialectical framework, an act of identification may be the instrument of both division and unity.

My purpose thus far has not been to indicate *what* the anecdotalists on my list believe, but, rather, to illustrate *how* they think and express themselves. I have characterized their common mode of thought and expression most broadly as analogic rather than digital. It may be objected that all science proceeds by analogy, and that social scientists in particular cannot do without guiding metaphors. True enough. When orthodox empiricists speak of "external validity," or of samples as "representative," or of conducting "replications," they are asserting or presupposing the existence of likenesses among phenomena. And it is true too that the "in-the-head" constructs of empirically minded theorists are often metaphorical. Terms such as "drive" and "attraction," for example, are "lifted" from the physical sciences.

Yet orthodox empiricists tend to label their "in-the-head" constructs as "intervening variables" and attempt to establish their "existence" by evidence of regularities between their antecedents and consequents, or by evidence of physiological concomitance. By contrast, anecdotalists tend to label their "in-the-head" constructs as no more than "useful fictions" that are not assumed to have direct physiological or behavioral counterparts. Moreover, anecdotalists are more apt to employ figurative analogies; that is, to select objects for comparison that are ostensibly quite dissimilar, and thence to identify their common structural or functional characteristics. Burke represents the extreme here by his use of the perspective by incongruity.

THE JUSTIFICATORY LOGIC OF ANECDOTALISM

It should be apparent at this point that anecdotalism does not lend itself very readily to reductionist translations or to tests of theoretical predictions by means of evidence that Scheidel can "see, feel, hear, experience, and measure." Occasionally, one finds ingenious empirical derivations of anecdotalist constructs, as in Laing's own system for monitoring co-orientations,[23] yet these operationalizations hardly lead to crucial "defining experiments" of the kind to which orthodox empiricists have traditionally aspired. It is, of course, possible to dismiss anecdotalists as muddleheaded and simply let things go at that. But then one should not allude to them as the firtile thinkers from whom we derive our theoretical predictions for empirical testing. Let us recognize the very real and perhaps unbridgeable hiatus between the theorists we find most heuristic and the forms of evidence we value as most rigorous.

I suggested earlier that anecdotalism has a justificatory logic of its own. I mean by that, not simply that it is capable of compelling our interest, but

[23] See: Laing, pp. 174-80.

that it is also capable of compelling our assent. Anecdotalism does not provide controls over errors of inference of the kind found in well conducted experiments, but it does permit "tests' 'or sorts of propositional claims.

The methods of "good" anecdotalism (much of it is horrible) are in some ways like those of the good novelist and in other ways like those of the comparative anthropologist. Watzlawick has supplied a clue:

> There are two methods of scientific explanation. One is to expound a theory and then show how ovservable facts bear it out. The other is to present facts from many different contexts to make obvious, in a very practical way, the structure that they have in common and the conclusions that follow from them. In the first approach, the examples are used as proof. In the second, their function is metaphorical and illustrative—they are meant to demonstrate something, to translate it into a more familiar language, but not necessarily to prove anything.[24]

Put another way, the orthodox empiricist derives narrowly circumscribed research questions from a broader theory, and then brings homogeneous sets of data to bear upon each question. The primary interest is in internal validity; external validity is of secondary concern. The anecdotalist derives general principles from his theory (rather than formally deduced hypotheses), and then brings heterogeneous examples to bear upon them. Internal validity is often in question, but one more frequently gets a sense of external validity.

The anecdotalists on my list tend to bring a great many diverse examples to bear upon their theories. Schelling draws support for his theories of deterrence and bargaining from the realms of the underworld, the military, law enforcement, psychopathology, child-rearing, business, party behavior, and maneuvering in traffic. Watzlawick draws on examples from psychotherapy, military history, intelligence work, animal communication, experimental psychology, Oriental fables, and children's literature. Bateson and Henry bring to their analyses of cases of psychopathology their common backgrounds in cultural anthropology. Burke travels between history and literature, religion and the arts, politics and routine human relations. We are persuaded, in part, that their principles have validity by their sheer range of application. To the extent that we can conceive of numerous counter instances, we become suspicious of their theories. In this respect, the tests we employ are much like those applied to the crosscultural generalizations offered by comparative anthropologists.

Yet merely supplying numerous examples is not enough. Much depends on the choice of examples, and on the way the examples are rendered. Good examples are not necessarily typical examples; they are often about aberrant or otherwise outstanding cases that serve to dramatize the point being made. Like the novelist, the scientific anecdotalist must be a good story-teller. Though he may make his field notes available, or the tapes or transcripts of

[24] Watzlawick, p. xii.

his psychiatric interviews, we recognize that there is necessarily a large gap between his propositional claims and his raw data. Much depends, then, on his capacity to interweave theoretical constructs and principles with ordinary language descriptions of the cases themselves. In place of dry, statistical summaries of findings (i.e., digital language), the anecdotalist must attempt to recreate in vivid, analogic terms the contexts of interaction, as well as the action itself. If the "characters" seem unreal, or if the "plot" seems strained, or if the anecdote is encumbered by too many extraneous details or asides (as in too many of Burke's tales) or if the explanation seems contrived, we become suspicious or turned off. If, on the other hand, the story appears to connect in some way with what we have already experienced or have come to believe; if the "melody" without is harmonious with a "melody" within;[25] then we are likely to achieve a kind of phenomenological understanding—what in German is called *Verstehende*.

CONCLUSION

Anecdotalists are often rightly condemned because they supply only anecdotes in support of claims amenable to more rigorous tests, or because they supply too few examples, or because their tales seem flat or their explanations unconvincing. My point in this paper, however, has been that there is such a thing as "good" anecdotalism; that it offers a common and coherent logic of justification which is consistent with its holistic, analogic, muddleheaded orientation; that it is particularly well suited to person-person relations (as opposed to person-object or object-object relations), and that it is often quite convincing. Admittedly, one might prefer more stringent tests of theoretical claims. Yet, given a discipline concerned with person-person relations, what are we to do? Are we to insist that all of our theories be fitted to our most rigorous methods for verification or falsification? I am afraid that that very tendency leads to an over-reliance on rather unimaginitive theories, and inhibits our empirically trained students from even reading some of our most insightful theorists. I would prefer at this point that we conceive of anecdotalism and orthodox empiricism as complementary approaches to both theory-building and theory-testing.

[25] The metaphor is Bateson's—from a lecture given at Temple University, April 17, 1976.

Evidence in Communication Research: We Are Such Stuff

When 'omer smote 'is bloomin' lyre,
'e 'eard men sing, from sea t' sea,
An' when 'e 'eard what 'e did desire,
'e went an' took, same as me.

ROBERT L. SCOTT*

YOU MAY NOT RECOGNIZE these verses immediately, even though you probably experience something of their particular nature that leads you to think of the British raj and its troubadour, Rudyard Kipling.

Kipling was saying that nothing much has changed in 3000 years. People loved and hated, ruled and ruined. And they celebrated their own motives, ways, and deeds. More than anything else, to Kipling, they celebrated, that is, they created a world of words in which they lived, drawing sustenance from the rich, symbolic compost all around them and adding to it their own deposits.

Of course Kipling was right: in reality nothing much did change between 900 B.C. and A.D. 1900. We refer to "reality" but our very saying is *symbolic*. We seem perpetually removed at least a half step from the *thereness* of whatever there is to experience. But at this level of abstraction, at least, I am making an argument that need not be pressed, not for most contemporary students of communication: reality is socially constructed and the products of the process as well as the materials of the process are symbols.

The "going and taking" that Kipling remarks on so boldly intrigues me. As communicating animals, we exist in tradition just as surely as fish exist in water. We can't help ourselves. But there is a difference between us and our milieu and the fish and theirs: we can help ourselves. We can grasp the paradox of our own existence; we communicate about the tradition, or relevant shards of it, and in doing so busily reconstitute it. We go and take.

And of course Kipling was wrong: a great deal changed between 900 B.C. and A.D. 1900. A great deal changed long before the end of the nineteenth century. In the first century, A.D., the Roman lyricist Catullus observed some verses of his lurking among those of a distinctly lesser person and wrote

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wryly, of his poor, *kidnapped* children. In doing so, he gave us our word for one sort of "going and taking": *plagiarism*. In Homer's time plagiarism simply was not possible. For that matter, it was not possible in Catullus', although it soon became possible. International copyright did not begin to be widely established until well along in the nineteenth century (the presses of early America maintained themselves to a fair degree by "pirating"). In the twentieth century, the ease of "going and taking" is such that highly controversial new laws are being enacted to control who shall have access, under what conditions, to the great store of symbolic materials heaped around us in the various neat packages to which we are used to giving names.

Do I write in the fashion I have affected here because the editor suggested that in developing a second panel on criteria, evidence, and theory what was needed was a couple of show humanists? (He didn't. You know he didn't.) The notion that scientists are not humanists troubles me. I tend to like the arguments of J. Bronowski when he argues the opposite.[1] After all, scientific theories are the work of human imagination, intelligence, and effort just as are poems and paintings, or they are nothing. It may be that the beauty of geometric propositions escapes most humans who have ever run across them, but Edna St. Vincent Millay told the generation before mine that "Euclid alone has looked on beauty bare." If few people are moved by the beauty that Euclid saw, few are moved by that that Millay saw, too. If numbers count, and we know that they do, the big battalions just now are on the side of Kiss.[2]

Accepting as a starting point for argument the proposition that Bronowski worked so diligently to establish, that science is humanistic, the humanism of science may be understood in several different ways. The way hinted at above is the aesthetic appeal of form. Bronowski, and others, strived to establish that doing scientific work and even understanding science inculcated human values, values that can only be called humanistic, if one is at all acquainted with the great tradition that that label stands for.

But there is another way in which science may be humanistic; that way is in taking humans themselves as the material of science. Humans are objects-in-the-world as surely as are any other objects, living or non-living, and the methods of the physical and natural sciences yield knowledge highly appropriate to the student of communication. Even though departments such as most of us populate tend to congratulate themselves on being more scientific than similar departments were in the old days, I would guess that students of *speech* were more apt to study the physics of sound or vertebrate anatomy in The First Days when at places like Iowa people like E. C. Mabie said, "Let our students paint sets, read poetry aloud, criticize speeches, and strive to

[1] See, for example, *Science and Human Values* (rev. edn., New York: Harper Torchbooks, 1965).

[2] Of course with the rapidly shifting populatrities of rock music groups, the one known as "Kiss" may be attracting little attention by the time this essay is printed.

understand stuttering." At least that's what the spinners of tales tell me; I knew not those precincts. But I did study sound and anatomy, never dreaming that Lee S. Hultzén was anything but a humanistic scientist, or a scientific humanist; of course Hultzén read Latin and kept asking me about the seminar on Cicero I was taking with Otto A. O. Dieter.

I am not suggesting that we should try to fan to flame those fires so long banked. After all, for most of us theatre has gone in one direction and speech pathology in another. We are left more specialized, or at least more limited in scope; and that's good, for we seem to have come to focus more and more on another sense in which humans are the material of science.

Humans are a peculiar sort of object-in-the-world: they know themselves as objects and believe strongly that other such objects are subjects also. The knowing object is a subject.

Human communication as a science, however, has been fixed on the ways of natural and physical science. For decades, speech, later speech-communication, and often, still later, simply communication found its scientists treating humans as subjects in the reduced sense of Ss. What a perfection of simultaneous familiarity and remoteness in an expression! *Ss* can only be taken as an oxymoron for *subjects*.

Again I hope to guard against misunderstanding. Although their style as scientists often makes me sad and sometimes makes me smile, I wish only insightful hypotheses, numerous Ss, clever statistics, and well controlled variables to our friends in the laboratories. I believe that their ways are apt to work better for physics and the behavior of lower animals than for that of human animals, but perhaps some useful law-like statements about communicative behavior will issue forth. If they do, we shall all be gainers.

Of course during the last half dozen years in our field we have heard a rising chorus of voices (and even some discords as they find one another off key) singing that the ways of conventional American behavioral sciences are not especially appropriate, perhaps not appropriate at all, to the study of human communication. At this juncture I should run in a long footnote, one that would fill an impressive space, but that gesture is so familiar and its content so repetitive as to be nearly a litany.

These essayists have been very long on philosophy of science and very short on such humble questions as *What shall count as data?* and *How shall we treat whatever data we gather?*

The makers of the special symposium—B. Aubrey Fisher, Thomas M. Scheidel, and Jesse G. Delia and Lawrence Grossberg together with moderators Gary Cronkhite and Jo Liska—seem to me, in a general sort of way at least, quite like the others I allude to.[8] And like them, they fasten on philosophy. Of course they would argue given the chance (and may even be inter-

[8] See: Winter 1976 issue of the *Western Journal of Speech Communication*, pp. 1-65.

preted to be so arguing) that fastening immediately on materials and tools is precisely where as scientists we have gone wrong and that, therefore, the corrective of a thorough discussion of the philosophic assumptions is mandatory. Moreover, from a fresh grounding, materials and tools will come soon enough. That sort of argument strikes me as both reasonable and pleasing so it must be right.| Nevertheless, I am going to try to take a step or two toward the more humble questions posed. Where angels fear to tread, as it were.

In the sort of science our most productive researchers have emulated, *time* does not count. Or, more accurately perhaps, *time* counts only in a very restricted way: as repetition. Each instance of anything, even the rock as I toss it in the air and catch it a second time, may be unique, but in the experimental sciences, the uniqueness of the individual instances must be set aside. When ways of setting aside are brought near to perfection, then hypotheses of what-is-what-and-how can be tested against repetitions predictively. Such testing has given us a sense of prediction so strong and clear that we have great trouble modifying it. That sense is usually expressed in statistical terms, but transcends these terms: say in advance with (near) certainty. In the reactions of subatomic particles in the nuclear accelerator or in the reactions of rats in the Skinner box, individuals do not count (unless one wants to pun and point out the individuals in the lab coats) and the patterns of reactions hold so stable that almost all errors may be attributed to a failure in procedure rather than of law.

The notion of aggregates, ignoring individuals, and looking for patterns of reactions had energized a good deal of study of socialized behavior, or at least of surrogates of socialized behavior, with the goal of upholding through prediction hypotheses that fit laws.

Right here is the eternal issue, however: must not the humans involved be reduced to something not-quite-human in the process of ignoring individuals, and so forth? I think so. However, our sense of being "higher" is quite tenuous and most of us are apt to agree that we are animals. For those who do agree, the hope (if that's the right word for it) of a productive behavioral science in its most reductivist sense cannot be entirely denied.

The hope is severely limited by noticing that humans must participate in living human lives, and even experimenters trust that it is human living that they study with the data gathered from their Ss. The more they control, the less human, in a very important sense, what they study becomes. A sense of participation is especially apparent if it is communicative behaviors that one wants to study.

If the subjects of study participate, then they will control the circumstances of the study to some extent, probably to a marked extent. But scientists cannot give up control of the circumstances altogether, for they must

participate also. Even if they try to be unobtrusive observers, what they take in will be what they take in and will have reference to something or another that is their purpose. Notice how the writers of the Winter 1977 symposium agreed that theory is prior. Only Scheidel suggested that data may be prior and I suspect that he had in mind mainly the general, interested experience that anyone can scarcely do without, since one must be a rather mature communicator before one will probably be at all interested, let alone able, to concoct any sort of theory of communication. Even Francis Bacon gave up the notion of gathering facts without any reference to a proposition, because without at least a tentative proposition no such things as "facts" could be gathered.

I am recommending that the new science of communicative behavior must re-conceptualize the notions of *control* and *prediction*. The central question is *What will lead to what? At* the moment, the substitutions that must be made for the place-holders in that question seem to be so numerous that any set of answers approaching completeness will be a vast and rich array of alternatives. We shall still be searching for regularities, but the regularities that we can only hope to find will be those that will lead us through a skein of possibilities. What we may be able to create are sets of statements that can be taken selecively to give coherent explanations of past communicative activities. Critical studies are already appearing in our journals and are billed, correctly in my opinion, as scientific studies of communication. Interestingly, a single recent issue of the *Quarterly Journal of Speech* presents three examples of the sort of studies I have in mind.[4] Although the authors work differently, all the studies are interpretive, and all are excellent examples of what Robert Jay Lifton calls, "the ever expanding use of the self as one's research instrument"[5]

We may hope for more; we may hope for prediction. Given some information about situations and persons in those situations, we may be able to make a series of if-then statements about what will lead to what. The series will not be predictive in the sense of saying of all the alternatives (if-thens), this one will occur and that will mean that of the next group of alternatives (if-thens), this one will occur, and so forth. In short, the prediction will not be a "hypothetical syllogism." Rather the prediction will be a number of alternatives relevant to the communicative situation such that once the instance has terminated one will have given in advance a limited set of statements that can satisfactorily explain the termination. Put another way, our critical activity will be forward looking. The mechanism of prediction in the sense here

[4] Thomas S. Frentz and Thomas B. Farrell, "Language-Action: A Paradigm for Communicaton," 333-49; Leonard C. Hawes, "How Writing Is Used in Talks: A Study of Communicative Logic-in-Use," 350-60; and Jesse G. Delia, "A Constructivist Analysis of the Concept of Credibility," 361-75, *Quarterly Journal of Speech*, 62 (1976).

[5] Robert Jay Lifton, *History and Human Survival* (New York: Random House, 1970), p. 5.

will be more like scenarios than hypotheses.

That's a shaky step toward "treating data" but what about *data* itself? What should count as data?

Perhaps some answers to the question have been indicated; I hope so. Just as I would not put aside the familiar notions of hypothesis, prediction, and control as treatment, neither would I contend that bits of behavior elicited in experiments in which the bits are operationally defined are necessarily irrelevant. But clearly I believe that their value in explaining communicative activity is severely limited.

The sorts of critical activity that I believe can be scientific must depend on the observation of communication in various natural settings. Some sociologists have been quite innovative in creating ways of carrying out the sorts of studies I have in mind. In general, participant observation, stimulated recall, and interviewing are altogether commonplace. I believe that we are going to have to develop specific sorts of activities that may be classified in these categories but that are particularly responsive to the phenomena of communication. Having said that, I must disagree strenuously with B. Aubrey Fisher who expressed such relief in his symposium contribution that we have at last outgrown our childish propensity to quibble about what "communication" means. "Quibble," of course, is a highly pejorative label. Fisher, in helping to birth that enchanting character the "double-interact," was working from a meaning of communication and was refining that meaning. I did not find that a quibble, although I did find it a curious phenomenon and rather hoped to be able to track it through Fisher's discussion because I thought that it just might be taken as evidence for communication research.

Since I take "intention" as an irrepressible aspect of communication, I am inclined to think that inevitably we must collect the reports of communicators as to what they were about.[6] I say that with no illusion that people neither dissimulate nor that they are necessarily perceptive reporters of their own experiences. We must learn to cope with these problems and, indeed, many researchers have struggled with them. They are scarcely unknown even when subjects become Ss.

What will informants be telling researchers about? The New Orleans conference in 1968 concluded that *messages* were the unique matter for our field.[7] But messages are not simply there; they are experiences. The joint making of messages is communication, and the parties to acts of making meaning must report in such ways as to aid the interpretation of communicative values, communicative impulses, communicative means, and communicative ends.

[6] See my "Communication as an Intentional, Social System," *Human Communication Research*, 3 (1977), 258-68.

[7] See: *Conceptual Frontiers in Speech-Communication* (New York: Speech Association of America, 1969), especially the opening of the "Recommendations Concerning Research Priorities," p. 33.

I do not mean to maintain that the observer of the communicative acts of others can report nothing useful. Scarcely. The *observer* and the *researcher* will continue to be roles that are to a fair degree identified. The observer needs the assistance of active participation in research by those observed. It is just in such a need that the observer of the phenomena of human communication is distinct from the observer of geological or astrological phenomena.

One more point.

Humans are beings blessed and cursed with traditions. Tradition is a curse in that it tends to make one blind to unique happenings; one is apt to see them, if one sees them at all, as conventional. But clearly, the same tendency is a blessing. That is, one can discover continuity in the ever changing flow of experiences. Tradition enables one to deal with the ever fresh phenomena of living.

Put a little differently: tradition lends stability to the dizzying confusion that challenges meaningful existence. Tradition provides those who participate in it with instruments for living constructively.

In general, we distinguish between tradition and science, although I have tried to depict the former in such a way as to bring out the features that make it like science. The scientist searches for stability, that is, science depends on the assumption that regardless of the undeniable freshness of each moment's perceptions there will be something that endures. That something is the pattern in which matter relates to matter. Both tradition and science lend stability and provide instruments for living. The difference between the two lies in the informality with which a culture effortlessly, that is, as the natural concomitants of the on-going functions of the institutions of a culture, inculcates traditions. Science, on the other hand, is ever a self-conscious effort. One stops, sorts out, and refers phenomena to some scheme of operation. Important in the operation of science is theory.

In what has been said, one might argue strongly that whereas tradition tends to be blind and rigid, science is ever false. It is false in its most essential aspects—the stopping, sorting out, and referring of phenomena to standard operations.

What I have called the "falseness" of science is at least disruptive of "truth" in concerns in which time plays the limited role. The sheer repetitiousness of phenomena that makes observation and calculation such a powerful pair of forces in astronomy and physics is not characteristic of the phenomena of human communication. The similarities observed are of a different sort; repetitiousness is not an accurate label for them. Conventions would be a better word.

It may seem that I am flirting, at least, with a contradiction. I said that tradition tended to be blind and rigid and now I am suggesting a sort of suppleness in human experience that makes the methods of natural and physical

science of limited appropriateness in dealing with the phenomena of the human sciences.

In considering that apparent contradiction, we must refer to the sense of time in human experience; its name is history. History is a teacher: it repeats itself. But that assertion needs countless caveats. At best, history allows one to chart a rough course. Further, history needs constant revision. In order to understand itself, humankind must recast the past, not because the events of the past change, but because the perspectives of the present change and, with those changes, the demands of the future shift also.

To put the matter a little differently: in spite of the blindness and rigidity, traditions shift. That is the clearest lesson of history.

What communication theory needs is much better interaction between the historical and the scientific efforts to serve it. I am at a loss, frankly, to specify the most promising sorts of "interaction" and must confess with the use of the quotation marks here that my term is question begging.

I am tempted to say that we must be more historical scientists; at any rate, that strikes me as more sensible than to say that we must be more scientific historians. The latter expression has some specific connotations in the field of history that are better eschewed here. But just as I argued that we should evolve fresh senses of "theory," of "control," and of "prediction," senses that seem to me to be already building in some current work in our field and in the social sciences generally, I have a notion that our sense of "scholarly" needs to evolve also. I am wedded enough to old scholarly values to want to preserve much that is familiar (there is the rigidity of tradition, perhaps), but I am utterly convinced that some of our younger colleagues are already moving with a scholarly spirit that is both scientific and humanistic, moving, that is, not with some tenuous compromise, but with a fresh and well unified amalgam.

I have refrained purposely from citing a number of sources and using some terminology that attracts me. I have done so not out of sheer laziness (I fervently hope) but out of a desire not to force people to choose up sides. The names of alleged leaders and the terms of favored ways and means have a tendency to become highly charged causes. Perhaps that tendency is not altogether bad; but, for the moment, at least, I have tried to avoid it.

My hope is to have made some general observations that may serve to remind a number of readers with somewhat diverse alignments that they share these rudimentary beliefs about evidence, criteria, theory, and human communication. And even more important, that there are some remarkable similarities in the thrusts in theory and research that may lead us to some quite basic reassessments of and reconceptualizations for communication research. As to its evidence: we are such stuff as dreams are made on.

The Rhetoric of Science And the Science of Rhetoric

HERBERT W. SIMONS*

IN THEIR INTRODUCTION to the 1977 *WJSC* symposium on admissable evidence, Gary Cronkhite and Jo Liska characterize as "especially interesting" the metacritical application of rhetorical tools for analyzing processes of social influence to scientific writings within our own discipline on the subject of social influence.[1] There have been a number of rhetorical analyses of scientific discourse, most of them offered from a debunking perspective. In keeping with Nietzsche's advice to critics that they practice "the art of mistrust," the debunker tends to be highly suspicious of those who speak in the name of scientific objectivity, his/her working hypothesis being that individual scientists, and even whole scientific communities, are as likely as not to exaggerate the worth of their efforts, and to promote in the guise of informing or proving.[2] In these contexts, the term "rhetoric" is generally used in a pejorative sense to characterize the discourse being analyzed as pretentious, evasive, or downright deceptive.

A prime example of the debunking approach, as applied to the social sciences, is Andrew Weigert's essay on the rhetoric of scientific sociology.[3] Maintaining that the claims of scientists to special expertise have been much exaggerated, Weigert charges behaviorally oriented sociologists with "identity deception" in their communications with colleagues, funding agencies, and students. In support of his thesis, Weigert ridicules what Jack D. Douglas

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[1] Gary Cronkhite and Jo Liska, "Introduction," *Western Journal of Speech Communication*, 41 (1977), 4.

[2] For an excellent discussion of the debunking approach, including Nietzsche's ideas on the matter, see: Peter L. Berger, *Invitation to Sociology* (Garden City, N.Y.: Doubleday Anchor, 1963), p. 30 cf. For a discussion of the debunking approach as applied to the rhetoric of science, see: Herbert W. Simons, "Are Scientists Rhetors in Disguise?: An Analysis of Discursive Processes Within Scientific Communities," in Eugene White (ed.), *Rhetoric in Transition: Some Points of Focus* (University Park, Pa.: The Pennsylvania State Press, *in press*). A recent essay in this journal by Philip C. Wander examines scientific discourse rhetorically. See: "The Rhetoric of Science," *Western Journal of Speech Communication*, 40 (1976), 226-35. See also: Jeanine Czubaroff, "Intellectual Respectability: A Rhetorical Problem," *Quarterly Journal of Speech*, 59 (1973), 155-64; and Thomas Szasz, *Ideology and Insanity* (Garden City, N.Y.: Anchor, 1969).

[3] Andrew Weigert, "The Immoral Rhetoric of Scientific Sociology," *American Sociologist*, 5 (May, 1970), 111-19.

has referred to as the ruling "hypothetico-deductive-statistical" paradigm of the social sciences.[4] He argues that scientific sociologists have been unable to explain away the "irreducible" logical gaps between their theorems, hypotheses, and operational definitions, or to justify fully what Jerome Schulman, *et al* have referred to as "The Worship of 'p' " (probability statistics).[5] Thus, from his perspective, the symbolic constructions that sociologists label as "scientific theories" can best be understood as status-enhancing forms of justificatory rhetoric, and the homage paid to statistics can be seen as a kind of secular prayer.

Drawing on Marx, Weigert further characterizes scientific sociology as a social movement of an intellectual elite, one that "has an opium for the people(as well as an identity-bestowing myth for its members."[6] To convince grant-givers that funds should be provided, appeals are addressed to public prejudices, scientific sociology is said to be indispensable to the solution of social problems, and the problems themselves are framed in the discipline's peculiar jargon. Meanwhile, textbook readers are first misled into believing that the discipline cumulates findings (the beginning survey text), are then hoodwinked into thinking that strict observance to methodological rules guarantees answers to the research problems being addressed (methodology texts), and are further duped into believing that no serious gaps exist between the language of research practice and the language of theoretical generalization (theory texts).

Weigert's arguments are overstated in some places and undersupported in others. He frequently fails to distinguish between inherent weaknesses in contemporary approaches to scientific sociology and bad practices by individual sociologists, and he is too quick to condemn all of scientific sociology on the basis of weaknesses in its prevailing paradigm.[7] Nevertheless, his rhetorical perspective is a highly provocative one, and many of his verbal thrusts will strike home to students of other social-scientific disciplines. The discipline of social psychology might be approached from a similar perspective, for example, albeit in a somewhat more tempered fashion.

In this paper I will reserve my verbal thrusts for the persuasion/attitude change area within social psychology—what I referred to in the title of this paper as the "science of rhetoric." Specifically, I am concerned with what I regard as the dominant social-psychological approach to the study of persuasion—the attempt to infuse high-level theorizing about so-called basic

[4] See: Jack D. Douglas, "The Rhetoric of Science and the Origins of Statistical Social Thought: the Case of Durkheim's *Suicide*, in Edward A. Tiryakian, ed., *The Phenomenon of Sociology* (New York: Appleton-Century Crofts, 1969), pp. 44-57.

[5] Jerome Schulman, Mary Jo Kupst, and Bernard G. Suran, "The Worship of 'p,'" *Journal of Communication*, 27 (1977), 38-41.

[6] Weigert, p. 112.

[7] Weigert goes so far as to suggest that sociologists should label themselves as "scientoid," i.e., tending toward the model of science," rather than as scientists, p. 118.

psychological processes with practical applications. Drawn from the physical sciences, this "covering law" approach has had considerable appeal for three decades now, but the record does not bear out its promise. I argue that the approach not only has not fulfilled the exaggerated claims that have been made for it, but also that it *cannot* fulfill those claims.

It was Nathan Maccoby, I believe, who first used the term "the science of rhetoric" to refer to the psychological theorizing and research on persuasion taking place at Yale and elsewhere.[8] The year was 1963, and as was true of the entire decade, it was a time of great promise for the persuasion area. Through careful evperimentation the Yale researchers had unearthed a series of empirical generalizations with titles like "the sleeper effect" that appeared to have the status of scientific laws. New theories bearing on persuasion appeared on the scene, and these, like cognitive dissonance and congruity theory, were generating hundreds of experiments by scholars in psychology, sociology, speech communication, journalism, and related disciplines.

Not only was persuasion a cumulative science, but it was also an eminently practical one at that, as Hovland and his colleagues had demonstrated in their wartime research for the armed forces.[9] At the same time that it was practical, moreover, it fit well within the mainstream of psychological theorizing about learning, perception, cognition, motivation, emotion, and other basic psychological processes. Thus, while Maccoby acknowledged the new science's origins in Aristotle's *Rhetoric*, he was quick to point out that the new rhetoric was solidly grounded in scientific method and was free of the value judgments and haphazard theorizing that characterized the humanistic rhetorical tradition.[10]

Thus far I have spoken of the heralded promise of the new scientific rhetoric rather than the actual record of accomplishments. I think it is apparent now that the dominant "covering law" approach to persuasion has been oversold—that scientists of persuasion have been pretentious in claiming to be able to provide general laws about basic psychological processes that could in turn yield reliable predictions about persuasive effects and thus serve as guides to practitioners.

The covering law approach has not yielded very many generalizations. Martin Fishbein and Icek Ajzen have concluded that, despite hundreds of studies conducted on persuasion, we are left with few generalizations that are not either circular, ambiguous, or just plain false.[11] Even the once- heralded "sleeper effect" has gone unconfirmed in the six efforts at replication

[8] See: "The New 'Scientific' Rhetoric," in Wilbur Schramm, ed., *The Science of Communication* (New York: Basic Books, 1963), pp. 41-53.

[9] See, for example: Carl I. Hovland, Irving L. Janis, and Harold H. Kelley, *Communication and Persuasion* (New Haven: Yale Univ. Press, 1953).

[10] Maccoby, p. 42.

[11] Martin Fishbein and Icek Ajzen, *Belief, Attitude, Intention, and Behavior* (Reading, Mass: Addison-Wesley, 1975), p. 478.

that have been reported since the original study.[12] William J. McGuire has conceded that the claims of progress during the fifties and sixties were largely illusory.[13] Anthony G. Greenwald has found support for mainstay propositions disappearing from view.[14] And Daniel Katz has characterized as "surprisingly small" the number of studies that supplied new information to a cumulative body of knowledge.[15]

In part, the problems in the area have come about as a result of gross violations of the canons of scientific logic. Fishbein andAjzen have taken their colleagues rather severely to task for having inconsistently operationalized independent variables, while neglecting the need for distinctions among dependent variables; for using single item measures of attitude in shot-gunned questionnaires; for relying heavily on theatrically staged deception experiments, appreciated more for their "cuteness" than for their capacity of generate consistent results; for the tendency to justify negative findings from loosely designed studies by means of *ad hoc* explanations; for failing to report statistically nonsignificant findings, while highlighting significant findings; and for drawing unqualified generalizations in reviews of literature from studies yielding inconsistent findings.[16]

Yet these examples of shoddy scholarship do not go to the heart of the problem. Although some defenders of the social psychological faith believe that their problems are remediable, there is considerable talk these days of a "crisis of confidence," as scholars come to grips with the possibility that some of their problems may be in the nature of unresolvable dilemmas.[17] How, for example, does one choose between laboratory experiments with poor external validity and field experiments with questionable internal validity; between complex deception experiments that are difficult to replicate and paper and pencil role-playing experiments that are prone to experimenter artifacts; between theoretically interesting definitions, whose novelty adds terminological confusion, and conformity with definitional conventions at the risk of reinforcing tired old ways of thinking; or between broad conceptualizations of independent or dependent variables that invite specious comparisons of research findings and narrow conceptualizations that then require

[12] See: Anthony G. Greenwald, "Consequences of Prejudice Against the Null Hypothesis," *Psychological Bulletin*, 82 (1975), 1-20.

[13] William J. McGuire, "The Yin and Yang of Progress in Social Psychology: Seven Koan," *Journal of Personality and Social Psychology*, 26 (1973), 446-56.

[14] See: Greenwald, pp. 1-20.

[15] Daniel Katz, "Some Final Considerations About Experimentation in Social Psychology," in Charles G. McClintock, ed., *Experimental Social Psychology* (New York: Holt, 1972), p. 557.

[16] Fishbein and Ajzen. See, in particular, pp. 11-2, 53-9, 114-17, and 513-19.

[17] Said McGuire, "The temple bell has tolled again, disturbing the stream of experimental social psychology research and shaking the confidence of those of us who work in the area," p. 446. See also: Allan R. Buss, "The Emerging Field of the Sociology of Psychological Knowledge," *American Psychologist*, 30 (1975), 988-1002; and Allen C. Elms, "The Crisis of Confidence in Social Psychology," *American Psychologist*, 30 (1975), 967-76.

identification of additional variables and distinctions among them?

Add to these dilemmas still another, first noted by Kenneth Gergen: that, as social psychologists disseminate the fruits of their research to the mass public, they set off the equivalent of a Heisenberg effect by changing the behavioral dispositions of the very persons about whom they have generalized.[18] Women resolve to become less gullible, for example, when they learn from social psychologists that they have been found to be more persuasible than men. In effect, no publicly reported generalization in social psychology can attain the status of a transhistorical scientific law because the generalization may be invalidated as a result of publication. Given this state of affairs, the social psychologist becomes little more than a contemporary historian of passing cultural fancies.

Among those with a purely academic interest in social psychology, there may be a partial solution to the problems just named. In a much-quoted article, Barry Schlenker has maintained that skeptics such as Gergen have been too preoccupied with "particular research examples which include specifics in their formulation and which were never intended to serve as transhistorical principles."[19] Granting that low-level empirical generalizations may not hold across persons, situations, cultures, and time periods, he nevertheless insists that social psychologists can abstract from those particulars to formulate universal theories and laws from which specifics can be deduced and explained. Thus, he argues, although the low-level finding that women are more persuasible than men may not obtain in another situation, culture or time, "an explanation of the finding might be obtained through deductions from a more general theory which hypothesizes that susceptibility to external influence is inversely related to perceptions of one's own ability to interpret complex events."[20]

Schlenker's call for higher-level theorizing about basic psychological processes has been a familiar cry among social psychologists, one repeated with great emphasis recently by Fishbein and Ajzen. Unfortunately, the quest for this holiest of grails is not readily realized.[21] What social psychologists generally mean by a basic process is an unseen, in-the-head, "operation" that is expressed as a metaphor (e.g., information processing, cognitive balancing) and is thus extremely difficult to pin down. Moreover, as Clyde Hendrick has observed, one social psychologist's basic psychological process is frequently another's lower-order generalization; the notion of "level of abstraction" is highly ambiguous. Hendrick adds that controversies about basic psychological processes, as between behaviorists and phenomenologists, are often objec-

[18] See: Kenneth J. Gergen, "Social Psychology as History," *Journal of Personality and Social Psychology*, 26 (1973), 309-20.

[19] Barry R. Schlenker, "Social Psychology and Science," *Journal of Personality and Social Psychology*, 29 (1974), 1-15, especially p. 3.

[20] Schlenker, p. 4.

[21] Fishbein and Ajzen, p. 520.

tively undecideable. How, for example, does one establish whether man is a "rational" or an "irrational" animal—a seeker after belief consistency or a seeker after evaluational consistency? Oftentimes, the quest for a simple answer to a question like this one leads psychologists to force-fit evidence to fit their theory.[22]

But assuming that stable, higher-order generalizations about basic psychological processes can be formulated, and granting their heuristic value, I still wonder how much use they can be to an applied social psychology such as the science of rhetoric. The more general the theory, asserts Gergen, the less it can serve the functions of deduction, prediction, and control. To the extent that the scientist of rhetoric is interested in making predictions about persuasive practices or in guiding practitioners, his or her most general theorems will be of least value—of less value, even, than unstable lower-level generalizations such as the finding that women are more persuasible than men. Moreover, to the extent that higher-level generalizations are forged from lower-order generalizations, they might themselves require revision as the facts on which they are based are shown to be ephemeral or artifactual.[23] Finally, as Hendrick has insisted, for higher-order generalizations to be applied to cases, the applied scientist would need to know, at a minimum, the precise rule by which a given basic psychological process combines with other such processes in a given case.[24]

In this paper I argued that the "covering law" approach to the science of rhetoric is not what it has been cracked up to be. Given the irreducible gap between higher-level theory and persuasive practice, and given the other dilemmas of choice which I have noted, I have great doubts whether improved designs, balanced reporting of results, and other such desiderata will be sufficient to make our Humpty-Dumpty whole again. Rather than placing faith on more steadfact applications of traditional canons, proponents of the new schientific rhetoric might do well to recognize the inherent limits of their discipline and refashion their statement of mission accordingly. Gergen, for one, has proposed a program for action by social psychologists that should be especially appealing to students of persuasion. It includes "revitalizing the enterprise of theory construction, developing our capacities as social critics and agents of social change, increasing our skills as social chroniclers, and

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[22] See: Clyde Hendrick, "Social Psychology as History and as Science: An Appraisal," *Personality and Social Psychology Bulletin*, 2 (1976) 392-403.

[23] The arguments which Fishbein and Ajzen use to support their own information-processing theory provide a good example. Having built their theory on laboratory and classroom experiments that are rife with experimental artifacts, they all too quickly explain away disconfirming evidence (for example, on verbal conditioning, on mere exposure, on errors in syllogistic reasoning, on dissonance-reduction mechanisms), and go so far as to justify reactive effects and restricted samplings "so long as the investigation is concerned with basic processes" (p. 122).

[24] Hendrick, pp. 392-403.

establishing a technology for actuarial prediction."[25] Each of these suggestions merits careful consideration. In another essay in this symposium, I offer a defense of a "softer" approach to the study of communication generally, which I label "muddleheaded anecdotalism."[26]

As of this writing, the scientific study of persuasion and attitude change is in a kind of limbo; a consequence, no doubt, of the malaise being felt generally in social psychology. To the discomfiture of those seeking simple answers, it may be that we will have to settle for context-specific empirical generalizations and *ad hoc* explanations for those generalizations. Whatever rhetorics prove dominant in the next decade, it is certain that they will need their debunkers. In its most profound sense, science is a history of errors, and it has been served well by those who have called attention to them.

[25] See: Kenneth J. Gergen, "Social Psychology, Science and History," *Personality and Social Psychology Bulletin*, 11 (1976), 373-83.
[26] Simons, pp. 21-28.

On Keeping the Faith In Matters Scientific

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EVIDENCE ABOUNDS that a segment of our field views the current status of communication research with alarm. Whether the specific issues involve the logic of theory construction, the methodologies employed in collecting and analyzing data, or the criteria used in assessing scientific evidence, the refrain is constant: prevailing research practices are fruitless and wrong-headed and our wrong-headedness will be remedied by an alternative approach that will lead to a true scientific understanding of human communication. In other words, the lost souls responsible for promulgating heathen research practices of old are urged to repent and to accept the word of the philosophical and scientific messiahs who have come to save them.

A new gospel is compelling when sinners are convinced that they have really sinned or when they are persuaded that it is indeed new and better than the prevailing theology. Unfortunately, we find the scriptures of alarmists jumbled. Rather than presenting a ringing reaffirmation of our own faith, we will content ourselves with pointing out several of the confusions and ambiguities encountered in the critics' writings. There are two kinds of confusions and ambiguities: those associated with practices which are characterized as the "reigning paradigm" and those associated with certain alternative paradigms that have been advocated. Although our comments are relevant to the problem of assessing scientific evidence, they also relate to broader issues concerning the epistemological moorings of communication research.

Indictments of the Reigning Paradigm: Or, Are You Positive That's a Positivist?

Disciples of the new research order frequently warn that stubborn adherence to prevailing practices forces the researcher to worship at the pagan shrine of an outmoded, discredited logical positivism. Despite the religious fervor of their remarks, it is often difficult to discern just *how* or in *what ways* these critics equate current communication research practices with positivism. Indeed, some skeptics have dismissed the effort to make such an equation as a red herring which relies on a naive or distorted view of logical positivism. For instance, Robert Bostrom stated in a recent paper that "close attention

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to most of these polemics reveals a standard tactic: the construction of a far-fetched and altogether unrepresentative set of assertions which are blandly identified as the essence of the positivistic position."[1] As we attempt to untangle some of the ways that prevailing research practices have been linked with positivism, it will become apparent that we are at least partially sympathetic with Bostrom's indictment of this practice.

In some cases, allegiance to logical positivism is equated with paying homage to the covering law model of explanation. Typically, the argument proceeds as follows: (1) logical positivism champions the covering law model of explanation; (2) the covering law model demands universal, invariant generalizations; (3) no such generalizations have been discovered in communication; therefore, (4) the covering law model is deficient; and (5) logical positivism is deficient.

This argument is patently specious, both on intellectual and practical grounds. Nowhere in the literature of logical positivism does one encounter the claim that covering laws constitute the sole acceptable means of scientific explanation. In his classic analysis, Carl G. Hempel distinguishes three types of scientific explanation, *two of which rely upon statistical or probabilistic premises.*[2] An astute critic of the law-governed approach to theory construction, Donald Cushman, has characterized the logical positivist position regarding explanation thus: "According to this view [positivism], laws enunciate the uniform concomitance of phenomena, the prototype instance of which is either a universal implication (all A are B) *or a problematic correlation* [italics ours]."[3] Simply stated, total allegiance to the covering law model of explanation is not an inherent feature of a positivistic perspective, nor does knowledge of one's position regarding the efficacy of covering law explanations permit his or her classification as positivist or non-positivist.

The practical flaw in the argument is that there are few, if any, practicing communication researchers who gauge the success of a research venture on whether or not a covering law has been identified. Indeed, to apply such an esoteric, admittedly unrealizable criterion to a research undertaking would be absurd; we count it as a step forward that some researchers have only recently ceased to attach caveats proclaiming that their results hold only for these subjects, observed under these conditions, at this point in time. Although we suspect that all communication researchers, regardless of their philosophical persuasion, would dearly love to identify a covering law, we believe they are astute enough to realize that "Theories have both explicit

[1] Robert N. Bostrom, "Epistemological Issues in the Study of Human Communication: Introspection, Objectivity, and 'Humanistic' Psychology," paper presented at the Southern Speech Communication Association Convention, Knoxville, Tenn., 1977, p. 16.

[2] Carl G. Hempel, *Aspects of Scientific Explanation: And Other Essays in the Philosophy of Science* (New York: Free Press, 1965).

[3] Donald P. Cushman, "The Rules Perspective as a Theoretical Basis for the Study of Human Communication," *Communication Quarterly*, 25 (1977), 31.

and implicit boundary conditions," and that to "assert that a lawlike statement is 'general' [does] not necessarily mean that it applies to all possible instances of a given phenomenon."[4]

Before turning to some other ways in which contemporary communication research has been characterized as positivistic, we want quickly to rule out certain philosophical principles of positivism on the same practical grounds used to dismiss the covering law model. Certainly, no one can accuse communication researchers of slavish adherence to reductionism; the literature abounds with concepts and constructs that cannot be "reduced" to sense-data statements. Similarly, we know of no practicing communication researchers who rely exclusively on the verifiability criterion of meaning, a narrow epistemic principle which we, like Jo Liska and Gary Cronkhite, take to be one of the cornerstones of logical positivism.[5] In short, if these are principles that mark a positivistic theology of research, we are hard-pressed to brand the sinners in our midst.

Lawrence Grossberg and Jesse G. Delia point out two other alleged shortcomings of a logical empiricist bias toward communication research.[6] First, they chide Thomas M. Scheidel for operating within the traditional, logical empiricist view [which] sees the observational descriptive phase of science as distinct from the theoretical-explanatory phase,"[7] a sin which they believe results from failure to adopt a Unitarian credo concerning the interdependence of theory and observation. To the extent that such a descriptive-explanatory duality is a hallmark of logical empiricist, Liska and Cronkhite cite Scheidel's own scripture to absolve him of this sin, noting that Scheidel actually argues for the interdependence of research phases and processes.[8] Furthermore, we believe that Liska and Cronkhite are dead right in asserting that Grossberg and Delia fail to distinguish clearly between *epistemological* and *explanatory* theories.[9] In terms of epistemological theory, it may, indeed, be impractical and counterproductive to make distinctions between observation and explanation. When it comes to explanatory theories, however, we confess to some confusion about the point that Grossberg and Delia are trying to make. If they are contending that the constructs and propositions of an explanatory theory are embedded in some prior epistemological commitment, then we grant the impracticality of distinguishing between observation and explanation at this level of abstraction. Likewise, if they are merely asserting that theories of communication should bear some correspondence with the

[4] Charles R. Berger, "The Covering Law Perspective as a Theoretical Basis for the Study of Human Communication," *Communication Quarterly*, 63 (1977), 9.

[5] Jo Liska and Gary Cronkhite, "Epilogue for Apologia: One the Convergent Validation of Epistemologies," *Western Journal of Speech Communication*, (1977), 61.

[6] Lawrence Grossberg and Jesse G. Delia, "In Response," *Western Journal of Speech Communication*, 41 (1977), 53-6.

[7] Grossberg and Delia, p. 53.

[8] Liska and Cronkhite, pp. 60-1.

[9] Liska and Cronkhite, p. 59.

way people communicate, we agree wholeheartedly; though as we shall note shortly, we suspect we disagree markedly with them on the extent to which contemporary communication theories satisfy this requirement. But if they are arguing that it is impossible (and, in fact, undesirable) to distinguish between the propositions of an explanatory theory of communication and the observational tests that are made of those propositions, we remain puzzled about and unconvinced of the utility of their position.

Finally, and perhaps most important, Delia and Grossberg seem to imply that a logical empiricist research stance suffers from its tendency to abstract and quantify data without adequate concern for "the density of meaning present in the pre-structured social world."[10] Such a charge is, of course, congruent with their view that a constructivist phenomenology provides the only path to the development of useful communication theories. For the moment, we will limit ourselves to two observations. First, the Delia and Grossberg indictment seems to relegate the researcher's identity to that of an automaton rather than a human being who himself or herself is privy to pre-structured social knowledge. To be sure, such knowledge is far from comprehensive, and the possibility of researcher bias must always be granted. Still, as B. Aubrey Fisher notes, and as we shall emphasize later, the alternative of relying on the reports of naive social actors creates another set of thorny problems for the researcher: for example, the actors may consciously distort their reports or they may be incapable of accurately reconstructing the cognitive factors that contributed to their choices and decisions.[11] Second, Delia and Grossberg accept the inevitability of abstraction and categorization; the crucial question is not *whether* researchers should impose order on observations and data, but rather *when* such an imposition should occur. Perhaps time and research experience will attest to the superiority of their procedural chronology; but, as we shall later argue in greater detail, the present case strikes us as far less open-and-shut than they have represented it.

Thus, despite all the rhetorical sound and fury, the destructive effects of logical positivism on current communication research seem to us to remain largely unspecified and unproven. As we have tried to indicate, certain charges rely on a naive, distorted articulation of the positivist position, while others construct a straw man caricature of current research practices—there are considerably fewer unsaved positivists in our ranks than the critics would have us believe. Finally, in those instances where current practices comport somewhat with a positivist theology, the evidence for the superiority of alternative doctrines remains equivocal. Although pluralism is preferable to a

[10] Jesse G. Delia and Lawrence Grossberg, "Interpretation and Evidence," *Western Journal of Speech Communication*, 41 (1977), 37.

[11] B. Aubrey Fisher, "Evidence Varies with Theoretical Perspective," *Western Journal of Speech Communication*, 41 (1977), 47.

dogmatic catholicism, the promises of the new prophets should be viewed cautiously and with the proper skepticism.

Indictments of the Reigning Paradigm: Or, How Does a Behaviorist Behave?

Critics of the prevailing research theology also charge that it is seriously tainted by its reliance on behaviorism. Unfortunately, this charge seldom includes a careful analysis of the sins committed by behaviorists. Recently, one of us has pointed out that the term "behaviorism" can be used in at least two senses.[12] We contend that in one sense of the term, few, if any, practicing communication researchers are behaviorists; while in the second sense, few, if any, are not. Furthermore, given the second sense of the term, we see no preferable (or, perhaps, even conceivable) alternative to behaviorism. The rationale for our contention will emerge as we distinguish between what we shall label *methodological behaviorism* and what we shall call *philosophical behaviorism*.

The term "methodological behaviorism" refers to a research posture which shuns recourse to intervening variables. The prominent American psychologist, John Watson, is generally regarded as the founding priest of methodological behaviorism; in fact, the phrase "Watsonian behaviorism" is frequently used to refer to single stage S-R models—that is, models that admit no mediating constructs between overt stimulus inputs and overt response outputs. By far the most distinguished contemporary methodological behaviorist is B. F. Skinner, whose theology of *radical behaviorism* also eschews reference to people's internal states. Although a recent poll revealed that Skinner is the most influential active psychologist, disciples of his position remain a distinct, albeit vocal minority in the field of psychology.

Among communication researchers, however, this minority approaches nothingness; we do not know of a single practicing researcher who is a committed methodological behaviorist.[13] Moreover, our quest for research reports that rely on this perspective in speech and communication journals has been largely in vain. Quite the contrary, recourse to intervening variable is commonplace: constructs such as "attitude," "ego-involvement," "cognitive dissonance," "communication apprehension," and "proactive attribution"—to mention but a few—are staple commodities of communication research.

Thus, to the extent that charges of behavioristic bias in contemporary communication research are intended to refer to a reliance on methodological behaviorism, they are empty of practical import. Lest we are misunderstood,

[12] Gerald R. Miller, "The Person as Actor—Cognitive Psychology on the Attack," *Quarterly Journal of Speech*, 62 (1976), 82-7.

[13] Perhaps the group that most closely adheres to methodological behaviorism are those individuals doing discourse analysis. We find this situation ironic, since many of them have preached vehemently about the evils of positivism and behaviorism. It is often useful to distinguish between how one chooses to *talk about* what he or she is doing and what one *actually does*.

we do not necessarily consider the absence of methodological behaviorists a good thing; indeed, the presence of a few such heretics might expand and enliven the research scene. Nevertheless, the fact remains that few communication researchers have been converted to methodological behaviorism; rather, they have consistently worshipped at the shrine of cognitive, affective, and motivational constructs.

Upon turning to a consideration of philosophical behaviorism, an entirely different picture emerges. The essence of philosophical behaviorism can be stated as follows: all we know, and in principle, all we can ever hope to know about people's mental states must be inferred from observing their behavior. Stated differently, people's mental states are inherently private; we can never gain direct access to them. Consequently, if we wish to "know" how they are *feeling*, we must observe how they are *behaving* and then use these observed behaviors as the basis for making inferences about their mental states. On some occasions, we elicit behaviors calculated to provide us with immediate inferential data; for example, an arden suitor may ask his beloved, "Do you love me?" and use the resultant "Yes" or "No" as the grounds for assessing her romantic feelings. Alternatively, we may observe a variety of behaviors thought to be indicative of an underlying internal state and then make the appropriate inference; for example, the suitor may observe how frequently his beloved accepts invitations for dates, how often she initiates communicative contacts with him, and how happy she appears to be in his company (a judgment which itself demands an inference from behaviors to mental states), and subsequently infer his love is or is not requited. When conflicting behavioral evidence exists, people tend to give greater credence to what they see than to what they hear, as attested to by the continuing popularity of such cliches as "Actions speak louder than words." But regardless of the behaviors employed, the basic tenet of philosophical behaviorism remains the same: knowledge of mental states must be inferred from observed behavior.

Philosophical behaviorism differs markedly from logical positivism and methodological behaviorism, both of which renounce any interest in mental states. We hold that practically all communication researchers are philosophical behaviorists, regardless of what other theological labels they may choose to adopt. For example, Delia and Grossberg's constructivist phenomenology is, in the last analysis, rooted in the behaviors of their naive social actors.[14] By the same token, the rule-following approaches of Cushman and Pearce must rely on behavior as a basis for drawing inferences about volition and choice.[15]

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[14] Delia and Grossberg, p. 33 *et passim*.

[15] See, for example, W. Barnett Pearce, "The Coordinated Management of Meaning: A Rules-Based Theory of Interpersonal Communication," in *Explorations in Interpersonal Communication*, ed. Gerald R. Miller (Beverly Hills, Calif.: Sage, 1976), 19-35; and Donald P. Cushman and W. Barnett Pearce, "Generality and Necessity in Three Types of Theory about Human Communication, with Special Attention to Rules Theory," *Human Communication Research*, 3 (1977), 344-53.

Indeed, any theoretical construct that is based on mediating processes or intervening variables can only be inferentially validated by recourse to observed behaviors, regardless of when, how, what kind, or how many behaviors are observed.

The preceding observation underscores the virtual inevitability of a philosophical behavioristic posture when undertaking communication research. Quite simply, we can conceive of no other feasible theology. Although communication researchers may be quite secular in their views about the relative merits of competing theoretical positions, the heuristic value of various communication constructs, and a host of other research issues, they will continue to share a common belief that behavior provides the primary evidential datum for making conceptual and theoretical inferences, for there are no other viable scientific alternatives at their disposal.

How, then, do we evaluate the argument that contemporary communication research suffers from behavioristic bias? In terms of methodological behaviorism, we view the argument as a blatant straw man; if anything, the situation is 180 degrees reversed, with communication research suffering from an almost total lack of interest in methodological behaviorism. In terms of philosophical behaviorism, however, we grant that communication research reflects a pervasive bias. We regard this bias as not only advantageous but also inevitable, because the only other option seems to be acceptance of scientific atheism. Nevertheless, we suspect our analysis will not satisfy some disciples of the new research order, who are likely to proclaim that they view behaviorism as a dehumanizing force in communication research which relegates humankind to unthinking passivity, rather than active participation. Consequently, when considering several of the alternative theologies that have been advocated, we will have more to say about the broad question of humanizing communication research, including some comments regarding the extent to which these alternatives accomplish this objective.

Managing the Meanings of New Paradigms: Assumptions and Facts

Having considered several alleged sins of the reigning paradigm, we turn to an examination of the possible paths to salvation offered by some of the new paradigms. In his discussion of the assumptions underlying his coordinated management of meaning perspective of interpersonal communication, Pearce states:

> The assumptions of the coordinated management of meaning may be summarized as stipulating (1) a diverse set of models for man, (2) a set of alternative modes of explanation for human action, and (3) a social science characterized as pluralistic and naturalistic.[16]

When amplifying these assumptions, Barnett Pearce distinguishes among three classes of action: controlled, influenced, and creative. Controlled actions

[16] Pearce, p. 18.

are caused, for example, by some kind of reflex action. According to Pearce, "Influenced behavior is structured by socialization and is appropriately explained by describing the rules which persons follow as they conduct purposive action."[17] An example of influenced behavior offered by Pearce is the turn-taking research of Starkey Duncan.[18] Influenced behavior is assumed to involve some kind of conscious choice. Finally, Pearce maintains that creative behavior is the product of individual choice but is not the behavior normally expected in the particular situation. Given this notion of creativity, exposing oneself in public would constitute creative behavior in most situations in our culture.

Although Pearce assures us that this new thology invites "a diverse set of models of man" and "a set of alternative modes of explanation for human action," he further opines that "Interpersonal communication probably includes elements of all three types of actions, but, for the most part, it consists of influenced behavior in which individuals allow their actions to be governed and guided by rules."[19] Thus, his gospel of pluralism is quickly supplanted by a dogma of monoparadigmism on the basis of an assertion not supported by one iota of data. How do we know that most interpersonal communication behaviors meet the definitional conditions necessary for assignment to the category of influenced behavior? If an entire research theology, complete with an elaborate epistemological and theoretical creed, is to be based upon speculation about the relative frequency of occurrence of three different kinds of behavior or action, one would like to determine whether speculation about the alleged distribution is, in fact, substantially correct.

Even if the sanctity of the assumption regarding the frequency of influenced behavior in interpersonal communication transactions is granted, considerable ambiguity surrounds the meaning of the term "influenced behavior." First, since influenced behavior is "structured by socialization," it seems plausible to argue that such patterns are caused or induced by individual socializing agents as well as the generalized other.[20] From an early age, children are "taught" to behave appropriately in various situations through a number of different learning processes such as role modeling and instrumental learning. Given the rule-governed approach to behavior, such learning processes and the theories associated with them are prime examples of the old mechanistic theology which is to be rejected. Yet all the new paradigm can offer as an explanation of how influenced behavior is acquired is to invoke the vague notion of "socialization." Can the new liturgy account for the acquisition of influenced action, which results from child-other interactions,

[17] Pearce, p. 19.

[18] Starkey Duncan, Jr., "Some Signals and Rules for Taking Speaking Turns in Conversations," *Journal of Personality and Social Psychology*, 23 (1972), 283-92.

[19] Pearce, p. 19.

[20] George Herbert Mead, *Mind, Self and Society* (Chicago: Univ. of Chicago Press, 1934).

employing principles *other than* those contained in extant, old paradigm learning theories?

A second ambiguity associated with the use of the term "influenced behavior" concerns the role of choice in such behavior. Specifically, the new disciples contend that the reigning paradigm views people's behavior as *determined* by laws and denies the possibility of volition. We assume that Pearce accepts this position both because of his endorsement of Romano Harré and Paul Secord[21] and because of his assertion that controlled behavior is rare relative to influenced behavior in interpersonal transactions. This latter assertion implies that persons generally feel they have a range of choices regarding their behavior in interpersonal communication episodes.

A number of problems surrounding the concept of choice are not addressed by Pearce; as a result, his litany is confusing on several points. Does "volition" mean the actual number of alternatives available to the interactant in a communicative situation or does it refer to the subjective feeling of freedom of choice so important in the production of the reigning paradigm phenomenon of cognitive dissonance? Moreover, is it not the case that awareness of rules and norms actually *limits* people's range of choice and increases the probability of the "appropriate" behavior so necessary for successful coordination of interaction? If so, then what appears to be influenced behavior is actually behavior *controlled* by rules and norms. If someone wishes to avoid being branded an obnoxious boor or a potential mental patient (and most of us do), he or she *must* observe the rules. This line of reasoning leads to the conclusion that a rules approach to communication does little in the way of restoring volition to a person "mechanized" by behaviorism; it merely generates the illusion of choice. If rule deviation is negatively sanctioned by other individuals or by the generalized other, and if rule conformity is rewarded by one or more of the same parties, we can discern little disparity between a rules approach to interpersonal communication and a reinforcement-oriented approach. Conceptually, the difference may amount to little more than the difference between Baptism by total immersion and Baptism by sprinkling.

Closely related to the problems associated with volition is the issue of awareness and self-consciousness. Pearce's endorsement of the Harŕe and Secord position and his statement, that when behavior is influenced individuals *allow* their actions to be governed and guided by rules, indicate he believes there is a certain level of self-consciousness or self-awareness present in individuals who are communicating interpersonally. This article of faith is both central to his coordinated management of meaning perspective and the methodological implications he draws from it. Unfortunately, it is suspect on a number of grounds. First, in some of the research used to exemplify influ-

[21] Romano Harré and Paul Secord, *The Explanation of Social Behavior* (Totowa, N. J.: Littlefield, Adams, 1973).

enced behavior, no evidence is presented to show that the persons involved in the interactions were *aware* of the rules they were following. Specifically, Duncan's study of turn-taking cues in dyadic conversations contains no such information.[22] Based on intensive study of videotapes of two dyads, Duncan found that a number of paralinguistic and gestural cues are associated with relinquishing control of the floor. Although several of these cues seemed to precede successful changes in conversational roles between speakers and auditors, Duncan reports no evidence that the communicators were aware of these turn-taking rules. Furthermore, given the nature of the turn-taking behaviors involved, it is unlikely that the communicators could accurately report the kinds of cues they used to signal their willingness to yield the floor. Not only do we question in what sense the participants in Duncan's study "allowed" their behaviors to be guided by rules, we also wonder where the *choice* resided in this particular situation. We doubt that persons in the study consciously thought: "I want to let Joe talk. Now what's the best way of letting him know I want to do this? Should I say, 'It's your turn,' or should I vary my vocal pattern at the end of a phonemic clause?"

A second difficulty with the tenet of self-awareness concerns the fact that people vary widely in the extent to which they characteristically attend to their self-presentations. Mark Snyder's research clearly demonstrates that awareness of self-presentation is not uniform across persons and leads to variations in interaction behavior.[23] Moreover, Shelley Duval and Robert A. Wicklund's theory of objective self-awareness posits that people oscillate between states of objective and subjective self-awareness depending upon the presence or absence of self-reflective stimuli in their environments.[24] Pearce's doctrine places communicators in the roles of persons who are always objectively self-aware. In this state, the person's self becomes the focus of attention, and he or she becomes concerned with possible discrepancies between ideal and actual selves. In the subjective state, attention turns way from the self and self-evaluative tendencies decrease. Concern for self-presentation and appropriate rule-following behavior implies a steady objective state, but Duval and Wicklund's theory argues that movements between the states are both rapid and binary in nature. Furthermore, when persons are functioning in the objective state, they often seek to become subjective to relieve uncomfortable, self-evaluative feelings. In short, both the Snyder research and the theory of objective self-awareness indicate that self-consciousness *cannot* be assumed to be either constant or at a high level in interpersonal communication transactions.

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[22] Duncan, Jr., pp. 283-92.

[23] Mark Snyder, "The Self-Monitoring of Expressive Behavior," *Journal of Personality and Social Psychology*, 30 (1974), 526-37.

[24] Shelley Duval and Robert A. Wicklund, *A Theory of Objective Self Awareness* (New York: Academic Press, 1972).

Because persons are *assumed* to have a relatively high level of self-awareness during interactions, it is further anticipated that they can provide researchers with valuable self-report data. This position is championed by Harré and Secord, in their chapter titled "Why Not Ask Them?," and is echoed by Pearce.[25] At least two reservations can be expressed about this naturalistic doctrine. First, we know Pearce is aware that self-report data are frequently gathered by reigning paradigm researchers. Asking an individual such questions as: "How often are you troubled with shyness?," "How often do you have difficulty talking with strangers?," or "To what extent do you see yourself as an opinionated person?" both allows the respondents to define their positions on a particular variable and to report their phenomenal experiences. When persons respond to such items, they are making judgments based upon their perceptions of their own evperiential histories in the content domains that are being tapped.

The preceding view might be objected to by some new theology advocates on the grounds that such self-report measures are structured by the researcher instead of the respondents; hence, the researcher might "miss" critical aspects of the respondent's phenomenal experience. Indeed, Delia and Grossberg have offered just this objection to structured measures and have emphasized the desirability of more open-ended measures.[26] The basic flaw in this argument is simply that at some point the researcher must *impose* a category system on the responses. Whether measures are structured or open-ended, they must be reliable and valid. A poorly constructed category system is just as unreliable as a poorly constructed structured measure. Moreover, even with open-ended measures, *some kind of question* must be asked or *some kind of verbal instructions* must be given. There is no guarantee that these questions or instructions, constructed by the researcher, will be any more or less efficient in tapping respondents' phenomenal worlds than a series of well designed, structured questions. We recall that this issue of open-ended versus structured questioning was raised early in the history of public opinion polling, and for reasons similar to those mentioned by Delia and Grossberg. Indeed, much "new" theology relies heavily on hoary texts.

Yet another argument against the use of structured items in self-report measures contends that even though the measure itself may be reliable, it could be irrelevant. We think this is a curious argument. Assuming that a researcher is measuring some set of variables to test a specific hypothesis, measurement of irrelevant variables should lead to disconfirmation of the hypothesis. For example, if cognitive complexity, as measured by an instrument that has been shown to be reliable and valid, fails to correlate with a variable as expected, the researcher has provided evidence that cognitive complexity

[25] Pearce, pp. 19-35.
[26] Delia and Grossberg, p. 37.

is irrelevant in accounting for variance in the target variable (assuming, of course, that the target variable is also measured reliably and validly).

Overall, then, we see little difference between the kind of research produced by new theology, open-ended measures, and by reigning paradigm studies. When one correlates various dimensions of cognitive complexity with self-disclosure,[27] social perspective taking,[28] or impression formation,[29] the kinds of conclusions that can be drawn differ little, if any, from the conclusions drawn by reigning paradigm research. A new theology can reasonably be expected to produce a scientific vision that is manifested through deeds, not verbal labels and symbolic distinctions.

A final issue related to respondent-based reports concerns the ability of persons to provide accurate explanations for their judgments and for their social behaviors. Based on an extensive literature review, Richard E. Nisbett and Timothy D. Wilson conclude that while a particular stimulus may produce a specific response, many respondents are: (1) unaware of the existence of the stimulus, (2) unaware of the existence of the response, or (3) unaware that the stimulus has affected the response.[30] These writers indicate that introspection about higher level mental processes may be impossible, and that cognitive structures which mediate the *verbal understanding* of phenomenal experience may differ from those which underlie complex judgments and behaviors. In a recent study, Richard E. Nisbett and Nancy Bellows demonstrated that respondents' subjective weightings of the relative influence of various factors on their judgments differed significantly from the actual weights the various factors displayed in their decisions.[31] Moreover, observers of the judges made the same kinds of errors as the judges themselves.

This area of research raises serious questions about the accuracy of information obtained when respondents are asked to testify why they behaved in a particular way. Nisbett and Bellows indicate that the kinds of explanations persons generate are based upon *a priori* causal theories which are frequently not, in fact, the actual explanations for their behavior.[32] It should be emphasized that Nisbett and his associates do not contend that persons are never accurately accounting for their behavior by verbal explanations, but rather that inaccurate explanations occur far more frequently than accurate ones.

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[27] Jesse G. Delia, "Attitude Toward the Disclosure of Self-Attributions and the Complexity of Interpersonal Constructs," *Speech Monographs*, 41 (1974), 119-26.

[28] Claudia Hale and Jesse G. Delia, "Cognitive Complexity and Social Perspective Taking," *Communication Monographs*, 43 (1976), 195-203.

[29] Jesse G. Delia, "Dialects and the Effects of Stereotypes on Interpersonal Attraction and Cognitive Processes in Impression Management," *Quarterly Journal of Speech*, 58 (1972), 185-97.

[30] Richard E. Nisbett and Timothy D. Wilson, "Telling More Than We Can Know: Verbal Reports on Mental Processes," *Psychological Review*, 84 (1977), 231-59.

[31] Richard E. Nisbett and Nancy Bellows, "Verbal Reports About Causal Influences on Social Judgments: Private Access Versus Public Theories," *Journal of Personality and Social Psychology*, XXXV (1977), 613-24.

[32] Nisbett and Bellows, p. 614 *et passim*.

New theology disciples might argue that the preceding evidence does not reduce the value of actors' accounts of their behavior because verbal understanding of phenomenal experience is all that counts anyway. It is difficult to reconcile this position with a perspective such as Pearce's coordinated management of meaning, which calls for both objective and subjective measurement of actors and cites exemplary research that is totally objective in nature—for example, the Duncan turn-taking study discussed earlier. If verbal understanding of phenomenal experience is all that is important to the communication researcher, why bother with objective measurement at all?

Lest we are misunderstood, we do not oppose asking respondents for reports of their phenomenal experience with either structured or open-ended measures. We have used both approaches in our own research and will continue to do so. What we do resist is the simplistic view that researchers who do only observational work or who employ structured self-report measures are somehow lost sinners who desperately need salvation. We believe in a genuinely ecumenical approach to communication inquiry, not one which is merely *apparently* ecumenical. When writers call for diversity but then inform us that other approaches are wrong-headed because they study statistically rare forms of behavior or because they do not use procedural techniques which permit elaborate answers by research participants, we are presented with the illusion of ecumenism without its substance.

A Concluding Article of Faith

Recently, Pearce has criticized the lack of communication within our "preparadigmatic" discipline.[33] To the extent that such a lack of communication exists, we attribute it not to the discipline's immaturity, but rather to the lack of ecumenical spirit which permits the theory and research of those who think differently to be not only tolerated but also supported and encouraged. Preoccupation with the fostering of sects of loyal disciples is currently a strong deterrent to such an ecumenical spirit.

Finally, we are weary of new theologies that rely on the following catechism:

1. The field is in a state of disarray.
2. Here is how the field can be saved.
3. Follow me.

Such theologies have been resurrected many times in the past. Usually, they consist of several arguments accompanied by little or no supporting evidence. Perhaps their disciples should be required to demonstrate the convergent and divergent validity of the alternatives they are proposing. Actual research findings should not only bear on the substance of the perspective, but they

[33] Pearce, p. 18.

should also illustrate what kinds of unique insights are provided by the alleged "cutting edge" paradigm. For we continue to believe that science thrives on a social action gospel; results, not rhetoric, will show the way to the promised land.